HOSPITAL LIBRARY

JUNE 1947

BULLETIN

OF THE NEW YORK ACADEMY OF MEDICINE

JUN 13 '47

Original Articles

by

JOHN A. RYLE

GEORGE H. HUMPHREYS II

MORRIS GREENBERG

and

OTTAVIO PELLITTERI

HOMER W. SMITH

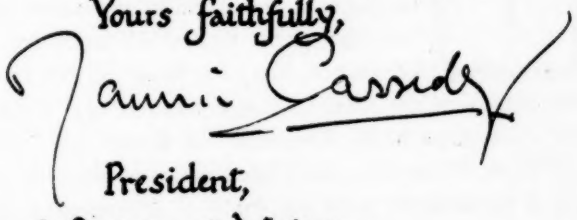
1847—CENTENNIAL OF THE ACADEMY—1947

perplexity, when all Nations of the Earth need spiritual, no less than physical, rehabilitation, our Profession is called upon to play a leading part in the restoration of the World to mental and physical health. This task is one in which the New York Academy of Medicine will add gloriously to its great achievements of the past Century.

The Royal Society of Medicine wishes the New York Academy of Medicine Prosperity, Courage and Success in its high endeavour, and it welcomes this opportunity of strengthening the bonds of friendship and co-operation which link our Institutions and the individual Members of them, thanks to our common language, traditions, aims and ideals.

Believe me, my dear Mr President,

Yours faithfully,



President,

Royal Society of Medicine.

This letter from the President of the Royal Society of Medicine of London to the President of The New York Academy of Medicine, was brought to this country by Lord Horder and was read by him on the evening of March 21st, just preceding the address which he delivered at the conclusion of the Institute on Social Medicine as part of the ceremonies commemorating the 100th Anniversary of the Academy.

BULLETIN OF
THE NEW YORK ACADEMY
OF MEDICINE

JUNE 1947

SOCIAL PATHOLOGY AND THE NEW ERA IN MEDICINE*

JOHN A. RYLE

Professor of Social Medicine, Oxford University

"Into those other fields of endeavour as we gaze, we see numberless close analogies to our own work. We see there another Pathology than that which our clinics and dead-houses teach us, yet a Pathology almost parallel in its teachings."†

THE term social pathology, in the sense in which we have come to use it, is not a new one. No one, perhaps, had the idea of social pathology more clearly in his mind than that great medical administrator, historian and man of science, Sir John Simon (1816-1904). The term was also employed by Alfred Grotjahn (1869-1931), the first Professor of Social Hygiene in the University of Berlin, who made it the title of a text-book. It has sometimes been given a restricted interpretation, linking it particularly with the study of delinquency, inebriety, vagrancy and crime, but for this restriction I can see no real justification.

Social medicine and social pathology should, as their names suggest,

* Delivered at the Centenary Dinner of The New York Academy of Medicine at the Waldorf-Astoria Hotel, March 6, 1947.

† Sir John Simon, "English Sanitary Institutions."

be considered respectively as the medicine and pathology of families, groups, societies or larger populations. Just as human pathology is the related science of clinical medicine—whether it be pursued by the morbid anatomist or the physician, in the laboratory or at the bedside—so may social pathology be viewed as the related science of social medicine, whether it be pursued in the office of the statistician or with the aid of socio-medical surveys or experiments, or of other more intimate and specific types of enquiry.

If clinical medicine is a comprehensive term—for it may imply both clinical practice and the theory or discipline necessary for the advancement of knowledge and the improvement of practice—social medicine is even more so. It embraces, on the one hand, the whole of the activities of the public health administration and of the remedial and allied social services, and, on the other, the special disciplines necessary for the advancement of knowledge relating to sickness and health in the community. Of these disciplines the more fundamental may be included under the general titles of social pathology and hygiology.

Historical Comparisons

We can carry our analogy further. Historically, the first developments of human pathology as a separate science were in the field of morbid anatomy. The first morbid anatomists were those great physicians—including, in my own country and my own school, Bright, Addison and Hodgkin—who, in the first half of the last century, realised the need for a closer study of morbid changes and processes and of the causes of death by means of the detailed *post-mortem examination.* Similarly the first scientific development of social pathology was the *social post-mortem examination,* as systematised, also in the first half of the last century, by William Farr and evolved by him as a mathematical refinement of the broad and historic social surveys of Edwin Chadwick. The two great branches of human pathology thus had their beginnings almost simultaneously. The social post-mortem examination employs statistical methods and techniques to reveal death-rates and their trends in the population, whether from all causes or from specific causes, and these rates and trends can be correlated with social factors and social change. It has this advantage over the individual necropsy: that it succeeds in shedding light upon the predisposing or *ultimate* (as distinct from the *intimate* or specific) causes of the prevailing social diseases,

as well as upon their incidence and distribution. The individual necropsy may reveal the extent and character and processes of a disease and the final cause of death, but it is silent about the circumstances which led to the development of the original morbid change.

The later progress of human pathology (as we ordinarily use the term) has required the advancement and co-operation of such sciences as bacteriology, immunology, protozoology, haematology and biochemistry. Radiology, endoscopy, physiological techniques and surgery have further assisted the study of the pathology of the living, and so have many careful clinical observations bearing upon the natural history and symptomatology of disease. Social pathology, too, has required the emergence and adaptation of associate methods—of methods of clinical, social and more specialist enquiry. Its epidemiological studies are concerned with the incidence and trends of diseases in the living community or its component groups, and with correlations of these with season, climate, race and environmental and social conditions. With a wider collection and fuller utilisation of statistical material relating to all the commoner types of disease and injury—and not merely to those due to crowd infection and connected especially with childhood, insanitary conditions and war—the horizons of epidemiology will by degrees be greatly expanded. Already analyses of official and other figures from various sources have considerably increased our understanding of some of our more chronic diseases and their distribution. In a more specific way *socio-medical surveys* are now likely to be applied in increasing measure to the refinement or extension of the findings of mortality and morbidity studies based upon official returns; to comparative studies of the health and sickness experience of different geographical, social or occupational groups within a population; to the investigation of the incidence and aetiology of particular diseases; or to the study of growth, development and sickness rates in infancy and childhood under varying economic and nutritional conditions. The method of survey thus makes use of "natural" experiments, just as the physician avails himself of "natural" experiments in studying the phenomena of disease in individuals. The survey has this advantage over the clinical investigation of the hospital ward, that it deals with larger numbers and can study simultaneously the incidence and manifestations of disease and health and borderline states in a community and relate them more directly by parallel enquiries to various causal influences. Hygiology—the study

of health and its causes—is clearly an associated interest of those who employ the survey methods of social pathology.

The third stage in the evolution of laboratory pathology witnessed the growth of *the experimental method*, employing animals and sometimes men. It has made far-reaching and valuable contributions to individual and special pathology, with important consequences for medical practice. The use of controlled *social experiments* in human populations also has important possibilities. Experiments of opportunity and planned experiments can both play their part. At times of rehousing, during industrial reorganisation, in schools or other institutions, and in the armed forces, opportunities arise of utilising changes of environment, employment or management and studying their effects on the health and sickness of partially closed communities. Sometimes a comparable population whose conditions remain, for a sufficient period, unaltered provides an additional check. The experiment of M'Gonigle and Kirby (1936) was a notable example of the experiment of opportunity and helped to direct immediate attention to the relative importance of nutritional opportunity and housing in maintaining or jeopardising the health of urban communities. These authors, during a time of economic depression, seized the opportunity of studying over a period of years one half of a slum population which was moved to a new housing estate, the other half remaining as a control. The health and mortality experience of the transferred population showed a significant deterioration. Higher rents and the cost of travel into town reduced the budget to such an extent as to leave too small a sum for sufficient and balanced diets.

In the services and in schools and in industry it should be possible, from time to time, to design and stage experiments having specific objectives and provided with adequate controls. It will never be possible to reproduce the precise conditions of the epidemiological studies of Greenwood and Topley on mouse populations, but human social experiments may nevertheless come to have considerable value for preventive medicine and social planning. Changes of human or psychological environment should be as important to study as changes in the material environment.

The Study of Groups

The groups or "populations" which are subjected to survey or ex-

periment at the hands of the social pathologist may be determined by age, as in the case of infants or schoolchildren or university students; by sex, as in the case of housewives or expectant mothers; by occupation, as in the case of factory workers; by age, sex and occupation, as in the case of army recruits; by geography, as in the case of a village or island community. A population may be studied in relation to its total health or sickness experience–as, for instance, in the case of an investigation of the growth, health and sickness experience of the preschool child, from birth to five years of age, which we are conducting at Oxford against the background of varying social opportunity. Another population, say of adolescents, may be studied in connection with its general nutritional status or, more particularly, in order to assess the frequency of a single manifestation of a specific deficiency. As an example of this I would mention a study of thyroid hyperplasia in adolescents (carried out by myself and colleagues for the Medical Research Council) and related with their geographical and geological environments and the iodine content of the drinking waters therein available. A "population" may also be composed of a number of persons suffering from the same disease (for instance, pneumokoniosis or peptic ulcer); in such cases, when aetiological factors or the results of treatment are under consideration, the enrolment of appropriate control populations becomes necessary.

Both branches of pathology, the individual and the social, require their special collaborations. Human pathology involves collaborations between the physician, the laboratory worker and others; social pathology between the physician, the statist, the medical social worker and others.

Further Analogies

I hope I may have succeeded in showing that there are close analogies to be drawn between the histories and the methods of these two sciences. Both have developed and will continue to develop their techniques for the investigation of disease in the living and the dead, and both have recourse to detailed observational and experimental methods. Both may require the assistance of mathematical methods and for social pathology (of which the statists, the epidemiologists and the great social surveyors have been the chief pioneers) biostatistics provide the basic science. The unit of study in the one case is the single human organism;

in the other the human population or social organism—whether this be as small as the family or as large as the nation. The pathological science in each instance is subscribed to by a variety of associate disciplines. In each case old methods must from time to time be modified or adapted, or new methods devised.

Briefly summarised, we may say that individual pathology deals with the quality and effects of diseases and, in practice, assists diagnosis and treatment, while social pathology deals with the quantity and causes of diseases and, in practice, assists prevention. Both sciences have their peculiar value, but as to which is the more likely in the long run to assist the saving of life and health and to encourage the more effective protection and development of peoples there can—from the evidence of history—be no real dispute. Their tasks, like those of individual and social medicine, should, surely, by now be more closely integrated.

The Case for Academic Status

In one respect the comparisons which have been drawn are incomplete. Human pathology, in its usually accepted sense, has acquired a status of its own in the medical faculties of all universities. Its associate sciences are commonly housed close together or even under one roof. Its numerous workers are given the opportunity to contribute to research and to the day-to-day teaching of undergraduate or graduate students. Outside a few great research institutes (I speak here of my own country), such as the London School of Hygiene and Tropical Medicine—which have served broader national and imperial needs and have had but a slender association with the life and work of the medical schools or with other scientific departments of the university—social pathology has, until lately, been accorded no position of its own. Its students, whether they work in the fields of public health bacteriology, of epidemiology and vital statistics, or in nutritional physiology, industrial psychology or other subjects, have tended to do so in various places and in detachment and without the advantages of a presiding and co-ordinating discipline. Team investigations of selected populations in relation to their total environment have seldom, until recent years, been undertaken, because organisation for the sampling or enrolment of populations for study was lacking. The epidemiology of diseases other than the fevers, venereal disease and tuberculosis, and more recently of cancer and the nutritional diseases, has scarcely been consid-

ered as a function of workers in the public health field. Its outstanding importance to clinical, constructive and preventive medicine notwithstanding, social pathology has not notably influenced clinical teachers and the regular instruction of the medical student.

Individual and social pathology, like clinical and social medicine, have developed along parallel rather than converging lines. And yet they have surely much to give to one another. Lacking the necessary time and associations for expansion, the medicine of the hospital ward and the research unit has been contracting its field and becoming by degrees an exercise in bedside pathology, pharmacology and therapeutic detail. The broader natural history of disease in man and man in disease has been too little considered. The too prevalent dissociation of academic medicine from psychological and sociological enquiry (without which no patient and no disease can be fully understood) engaged the interest of Cabot (1918) some years ago and has been well commented upon by Canby Robinson (1939), Richardson (1945) and others in your country. Dr. René Sand (1938) in Belgium has been a leading pioneer in the combined disciplines.

It is curious that aetiology, in its wider sense, should have so far lost the interest which it had for the older physicians. While specific agents are still assiduously sought for, the contemporary neglect of more comprehensive enquiry—taking into account the influence not only of specific factors and of age and sex and race and heredity, but also of economic circumstance, domestic environment, occupation, nutrition and education—would, I believe, have attracted the adverse comment of such great physicians as Ramazzini, Trousseau, Fagge, Gull, Wilks and Osler, were these great men to return to us to-day.

These modern trends in medicine have not been good for the training of the undergraduate student or for the thought of his teachers. Have we provided anything better for the graduate student? Have we helped him to a just appraisement of his functions and his problems? In general, the newly qualified doctor embarks upon his career steeped in the ideas of individual pathology, moderately well versed in therapeutic techniques, and with a smattering of psychology, but almost ignorant of social pathology; knowing little of the incidence of diseases and their mortalities and secular trends or of the social factors which are in part or whole responsible for their inception or continuance. His interest in the frequency and the reasons for the frequency of the more prevalent dis-

eases and injuries—whether lethal and crippling, or crippling but not lethal, or of less serious type—has scarcely been awakened. The possibility of preventing them has been too little discussed with him, whether at the bedside or elsewhere. In regard to some important groups of diseases he is actually misled by terminology and his text-books. Cholera, plague, malaria, the dysenteries, leprosy, hookworm and beri-beri have been classified for him as "tropical," or, perhaps (if they have too recently been familiar nearer home) as "sub-tropical" diseases. And yet they have all occurred and some of them have even flourished in Great Britain and European countries and the temperate zones of the Americas at a time when dirt, poverty, squalor and malnutrition, and ignorance or neglect of sanitary laws were conspicuous among the attendant social influences. These great endemic diseases of backward populations still prevail in India, China and Africa. Nearly all of them are pre-eminently "social" diseases and due to alterable social causes. They are, strictly speaking, linked rather with a stage of historical development than with latitude or climate. The adjective "tropical" (if we except diseases due to parasites and vectors which are only found in the tropics) is a misnomer.

Other and more chronic diseases now prevalent in Europe and the West—peptic ulcer, cardio-vascular disease, cancer, the chronic rheumatic diseases, the visceral neuroses, the psychoneuroses and accidental injuries—also have their epidemiologies and social aetiologies, but this has not been made sufficiently apparent to the mind of the student or practitioner. And yet if a graduate, stimulated by an interest in preventive medicine, desires to enter the public health service, his training for a special diploma has directed his thought too exclusively to the study of fevers and the immediate material environment, to sanitary law and engineering and to public health bacteriology and chemistry. The *living human community* has been insufficiently considered as an object worthy of study, and, outside his brief apprenticeship in vital statistics, he has not been greatly encouraged to consider fundamental methods of enquiry as important to preventive and constructive medicine as histology, physiology and biochemistry are to clinical medicine.

In England the opportunities for a research training and career in social or preventive medicine have in the past been very few and to be found chiefly at such large institutes as the Lister Institute or the London School of Hygiene. The former is mainly devoted to scientific

investigation and standardisation at the laboratory level. The latter, while attracting famous men, providing our best graduate diploma courses and fostering first-class laboratory, epidemiological and environmental research has not directly fertilised the medical schools. New experiments and opportunities in other settings are now needed.

"Social Medicine" and "Public Health"

I have scarcely as yet had time to familiarise myself with the organisation of research and teaching in the field of preventive medicine in your universities, and it may be that I have overemphasised the distinctions between what we have long called "public health" and what we now call "social medicine." The main differences, however, would seem to be these:

1. Public health, although in its modern practice attaching an ever-increasing importance to the personal services, for a long time and at first for very sufficient reasons, placed the emphasis on the *environment*. Social medicine, deriving its inspiration more from the field of clinical experience and seeking always to assist the discovery of a common purpose for the remedial and preventive services, places the emphasis on *man*, and endeavours to study him in and in relation to his environment. Furthermore, the immediate material environment, in the shape of housing, drainage and water supplies, is to-day extended to include the whole of the economic, nutritional, occupational, educational and psychological opportunity or experience of the individual or of the community.

2. Public health, in the first instance, and again for obvious reasons, has been largely preoccupied with the communicable diseases, their causes, distribution and prevention. Social medicine is concerned with all diseases of prevalence, including peptic ulcer and the chronic rheumatic diseases, cardio-vascular disease, cancer, the psychoneuroses and accidental injuries—all of which have their epidemiologies and their correlations with social and occupational conditions and must ultimately be considered to be in greater or less degree preventable.

3. Where hospital practice (as distinct from preventive theory and practice) is concerned, social medicine properly takes within its ambit the whole of the work of a modern almoner's department; this includes social diagnosis and social therapeutics—the investigation of conditions, the organisation of after-care and the readjustment of the lives of individuals and families disturbed or broken by illness. The almoner or medi-

cal social worker also has an important part to play in teaching and in the follow-up activities of a clinical research unit.

In brief social medicine extends the interest and alters the emphasis of the older public health, just as social pathology extends the interest and alters the emphasis of earlier epidemiological study.

Accepting these changes of emphasis, can a case be made for the provision of a status for social medicine and pathology in our universities and for research departments working in close association with their medical schools? If so, how should their functions be determined or how developed from those pertaining to existing schools of hygiene and preventive medicine? What nucleus staff will they require? And, finally—and to this question the title of my discourse requires a reasoned answer—what may their influence be upon the evolution and progress of the Medicine of our coming age?

New Trends and Prospects

In attempting to answer the questions which I have posed, it occurred to me that a brief account of the Oxford experiment and of some other developments in England might serve our purpose better than theoretical discussion or a mere statement of personal views. I can deal but briefly with descriptions here, but elsewhere I shall give a more detailed account of our experience.

What I have referred to as the Oxford experiment has been in progress for barely four years. Nevertheless, it can be claimed that it is beginning to take shape satisfactorily, notwithstanding its difficult beginnings while the war was still in progress. The staff of my small Institute includes physicians, statisticians, a radiologist and a radiographer, a medical social worker, attached workers with nutritional and epidemiological interests, secretarial and clerical assistants, and (together with a regional Bureau of Health and Sickness Records which also comes under my direction in the same building) a unit served by expert records officers, transcribers, coders and technical workers in the Powers-Samas machine room.

With the assistance of the medical officers of health for Oxford city and county, a tuberculosis officer, a physician for maternity and child welfare, a school medical officer, a public health bacteriologist, a factory medical officer, a chief sanitary inspector and the almoners of the Radcliffe Infirmary—all working in the Oxford area—I and my

statistical colleague, Dr. W. T. Russell, have undertaken responsibility for the whole of the undergraduate teaching in social medicine and public health. We provide a course of lectures, socio-medical case-conferences and field visits running through the three terms of the academic year. At the case-conferences we present cases as living texts bearing upon the correlations of disease with social circumstance; the cases are shown in turn by the house-physician or a senior student and by the medical social worker, and are then discussed by the professor and the class. Teaching of this kind helps to forge the essential link between the two disciplines of clinical and social medicine. The lectures, primarily intended for medical students in the clinical period, are open to graduates, medical social workers, health visitors and health visitors in training. The course has replaced the former statutory course of detached public health lectures occupying a few weeks of the year only. The inculcation of principles and the inter-relations of social and clinical medicine seem to me more important at this stage than instruction in the details of sanitary technique and law and administration. Specialist training in all subjects should surely be reserved for the graduate period.

We hold no special graduate or diploma course. I regard the best type of graduate opportunity as that provided (as in other university departments) for a small number of men or women holding senior or junior research assistantships, each working on a particular problem but all sharing in the community of interest which the Institute provides. Our research programmes include, or have included, a continuing child health survey between birth and 5 years and covering all social groups; a statistical study of the stillbirth rate in the population in relation to social and nutritional factors; a study of the effects on parturition of residential ante-natal rest; a study of adolescent thyroid hyperplasia in relation to a positive or negative history of endemic goitre and the iodine content of local waters in various parts of rural Britain; a continuing study of occupational morbidity and accidents in neighbouring factories; a study of peptic ulcer in factory populations; studies of endemic fluorosis and of the fluorosis hazard in the neighbourhood of certain industries which emit fluorine-containing smokes; and a long-term study of student health.

Since 1943 three Departments of Social Medicine have come into being, at Oxford, Birmingham and Edinburgh—the first two with endowments provided by the Nuffield Trust. Three additional Chairs and

Departments of Industrial Medicine—and in an industrial country occupational health and sickness furnish a large part of the problems of social medicine—have also been created by the Nuffield Foundation at Manchester, Glasgow and Durham. Chairs of Child Health have been established in London, Liverpool, Newcastle, Birmingham, Leeds, Sheffield and Edinburgh. The beginnings of life, for very obvious reasons, are particularly worthy of detailed socio-medical investigation. Those chairs and departments for which the Nuffield Foundation makes provision are given endowments for an experimental period of ten years. Thereafter, if they make good, it is to be hoped that they will become a permanent responsibility of the universities concerned.

The case for social medicine as a subject worthy of support in a university setting would seem, in fact, already to have been accepted in Great Britain, and the report of the Goodenough Committee (1944) has done much to foster the idea. I would, however, submit that if these departments are to be fully effective and successful in their contribution to teaching and research and eventually to the improvement of practice and services, two things are necessary. First, they must—with critical regard for the quality of their methods and the maintenance of standards—develop their related disciplines, the methods and techniques of social pathology and hygiology. And secondly, in order to secure ready access to the populations in which they are interested and the collaboration of those immediately responsible for the health of these populations, they must establish a close and friendly liaison with the departments of public health, the medical and welfare departments of industry, the education authority and the medical officers of the school services, and others. They should also maintain an association with the hospitals and science departments of the university cities in which they are established. For instance, clinical medicine and departments of both social and physical anthropology may well have overlapping interests with an institute of social medicine. Any tendency to academic isolation would at once render abortive the work of a department devoted to socio-medical enquiry. Teaching in social medicine and pathology should be available for the student throughout the clinical period of his training and should wherever possible be related to his bedside teaching.

The expansion or modification of existing programmes by established schools of hygiene and the essential two-way linkages with the clinical

worker and with the field worker in public health or industrial medicine may not be easy to bring about. Nevertheless, the experience which we have gained during the last four years at Oxford compels me to the belief that there is a case for other experiments—not necessarily identical in type—in the medical faculties of other universities. Only time can show how far our English programmes will justify themselves. Appropriate research staff and men with the training and outlook necessary for the post of director in such departments are not at present plentiful. Where such senior posts are in question, long clinical and teaching experience is probably at present more important than public health or industrial experience, for socio-medical investigations are largely concerned with human assessments and socio-medical instruction should be related constantly to patients and their problems and to the natural course and consequences of disease. It is, however, much to be hoped that public health men will also be attracted into the field and that students from departments of social medicine will enter the public health service.

In the matter of staffing I would say that whatever specialist additions may later be found necessary, the personnel of a department should include physicians, biostatisticians, medical social workers and, probably, a radiologist. The need for a nutritional or occupational physiologist or a social psychologist will be determined by the types of investigation undertaken or by the availability of colleagues working in neighbouring departments. For instance, a study of postural defects in school and pre-school populations—and how can we ever advance our knowledge of their aetiology by studying the end-result material which reaches hospital?—would clearly require the collaboration of an orthopaedist, but it is unlikely that a department of social medicine would have a place on its staff for a whole-time specialist in this category. It might well, on the other hand, employ one or more whole-time paediatric research workers, for we cannot over-emphasize the importance of the study of early life and its special hazards within its varied social settings and of ostensibly healthy children. The study of occupational problems in industry or the home might call for quite different types of enquiry from those at which I have hinted. For instance, in the particular problems relating to man and the machine, or the housewife and her tasks, the physiologist with wartime experience of human adaptation to the tank and the aeroplane might supply valuable experience.

Under our new National Health Service the country will, for the purposes of administration and the hospital service, be divided into a number of regions. Where possible, a university with a medical faculty will provide a parent hospital for the region and help to influence the quality of the service and to provide what might be called its medical intelligence bureau. It would seem to me that a university department of social medicine could come to have high value, both academic and social; could maintain a co-ordinating interest in the vital statistics and survey work of the area, and assist in the study of the particular hazards for the peoples of the region created by main industries or retarded social development. Retaining academic liberty and without coming within the immediate sphere of state administration, it could yet have a healthy influence upon the conduct of an evolving service, could help to encourage salutary emulation as between regions, and—with the aid of its statistical and records departments—serve perhaps, as a local Registrar-General's office in miniature and provide therein a training for biostatisticians and research apprenticeships for physicians and medical social workers.

The Future

I come lastly to the most important and yet the most difficult question which I have set myself to try and answer. Speculation is not my habit, but this great centenary gathering, this hour in history which we all hope to look back upon as a moment of pause between two eras—a bad one and a better—tempt me to invite my native optimism and emergent ideas to go into partnership for the occasion. "What," I have asked, "may be the larger influence of these new directives for medical and social thought and action? How, possibly, will they assist the general evolution of Medicine in the new age that lies ahead of us?"

Our great contemporary biologist and humanist, Julian Huxley (1944), has discussed the transition now in progress from the age of "economic man" to the age of "social man." Our profession, which is so particularly concerned with man and his welfare, must assist this transition with all the scientific and humanist wisdom at its command. Hitherto, our science, like our practice, has evolved along individualist lines. Whatever the several countries may do with regard to the modification of their systems of practice and of service, it seems to me that the scientific study of health and disease in man—the most complex of

all social animals—must henceforward concern itself to an ever-increasing degree with the interactions and correlations of disease and health with changing social circumstance. Socially, industrially, politically, we are creating a new age. With it, inevitably, we alter the whole character and distribution of diseases and set ourselves new problems for solution in the fields of medical science, practice and administration.

Some of my friends have rebuked me for leaving the clinical fold. I reply in effect that I have merely taken the necessary steps to enlarge my fiield of vision and to increase my opportunities of aetiological study. My allegiance to human medicine is in no whit broken. I wish I could convey to them and to you some of the sense of stimulation and rejuvenation that my close association with statisticians and medical social workers and with men and women in the public health and industrial health services has brought to me. Thirty years of my life have been spent as a student and teacher of clinical medicine. In these thirty years I have watched disease in the ward being studied more and more thoroughly—if not always more thoughtfully—through the high power of the microscope; man in disease being investigated by more and more elaborate techniques and, on the whole, more and more mechanically. Man, as a person and a member of a family and of much larger social groups, with his health and sickness intimately bound up with the conditions of his life and work—in the home, the mine, the factory, the shop, the office, at sea or on the land—and with his economic opportunity, has been inadequately considered in this period by the clinical teacher and hospital research worker. The medicine of the teaching schools has, as I have suggested, undergone a gradual conversion to a highly technical exercise in bedside pathology and therapeutic method. The morbid "material" of the hospital ward consists very largely—if we exclude the emergencies—of end-result conditions for which, as a rule, only a limited amount of relief repays the long stay, the patient investigation and the anxious expectancy of the sick man or woman. With aetiology—the first essential for prevention—and with prevention itself the majority of physicians and surgeons have curiously little concern. Nor have they at present the opportunity, nor yet the appropriate types of training or assistance, requisite for the study of aetiology or prevention. Their material is mainly selected by four factors: the gravity, the difficulty or the rarity of their cases, or their suitability otherwise for admission to a hospital. Some of the most common diseases, the less lethal diseases and

the beginnings of disease are even considered as providing "poor teaching material." Health and sickness in the population and their possible correlations with significant and measurable social or occupational influences are outside their province.

Reverting, for a moment to the question of the prevalent non-communicable diseases, our modern endemics, let us take as an example a particular type of disease—one in which I have long been interested: gastric and duodenal ulcer. We have watched these diseases—which were at one time, judging by earlier records of the clinic and the dead-house, relatively rare—becoming in the course of two generations two of the most common of all. Their incidence is still rising. If the combined total of living persons who have or have had an ulcer in America and the British Isles could be computed, they would be found to number not tens or hundreds of thousands but millions. Every endeavour, orthodox and unorthodox, medical and surgical, has been made to discover a cure, but with no very outstanding or encouraging results. Animal and laboratory experiments have carried us but a very little way in the assessment of cause. And yet the causes (for ulcer will probably find its place among the "multiple stress" diseases) must have been developing contemporaneously with rising incidence. A disease which was once rare can become so again. Until we study the victims of the disease at first hand and in relation to their work, their total occupational experience, their communities, their food, their habits and anxieties, and their innate predispositions, are we likely to obtain the answer to our question?

And here, briefly, are some other examples from among our outstanding socio-medical problems of to-day. One of my statistical colleagues has lately shown that there is strong evidence of relationship between the stillbirth rate in England and Wales (at present depriving us annually of some 20,000 potential citizens) and the nutritional experience of a population (Sutherland, 1946). Before the war the infant mortality at 3—12 months in the unskilled worker group in England and Wales, a mortality sustained largely by environmental conditions, was four-and-a-half times that in the professional classes. Rheumatic heart disease, like tuberculosis, has a close correlation with poverty and crowding. The mortality from gastric and skin cancers among the working classes is twice as high as that in the professional classes. Angina pectoris —and coronary disease is taking a higher toll each year—has a death-rate

among doctors that is twice as high as it is among bank and insurance officials and nearly twelve times as high as it is among agricultural workers. Facts of this kind should surely be known to every medical student. Until they are known and widely discussed and subjected to further analysis, the appropriate education of the profession and the public and the enlightenment of the legislature cannot follow and necessary protective measures and social planning must inevitably be delayed.

One of the most disturbing thoughts in our present age is the long lag between the arrival of new knowledge and necessary action. In some directions you tend to move faster in America than we do. We still have to confess that milk pasteurisation in England is far from universal. We payed for this delay recently with some 600 childhood deaths from bovine tuberculosis and 2,000 non-fatal cases in one year. Although the diphtheria immunisation crusade is now steadily extending, we have also to admit that the deaths from this preventable disease among children during the war years exceeded all those due to enemy action. In all countries infectious disease and poor nutritional opportunity take too high a toll of the working classes. In England we have shown during the war what a fair distribution of food can do even in otherwise exacting circumstances. With our declining birth-rates and ageing populations in Europe and the West, we cannot afford to neglect the study of how we may further reduce, in particular, our ante-natal, infantile and childhood deaths.

Prevention or Cure?

For a very long time we have accepted the old adage "Prevention is better than cure." In our new era the belief in it—for of its truth there can be no doubt—must be made ever more manifest in our research and its directives and in our teaching. The most conspicuous interest of the student ten or twenty years hence will, I hope, no longer be in the rare or difficult and too often incurable case, but in the common and more understandable and preventable disease. May the daily questions on his lips become not "What is the treatment?", but "What are the causes?" and "If preventable, then why not prevented?"

The study of the ultimate causes of disease—the protocatarctic causes, without which the specific factors can never find their opportunity—goes hand in hand with the study of the causes of health, and how much we have still to learn of the meaning and measurement of health. When

social pathology and hygiology come into their own we may witness a return—but this time with fuller scientific authority for the guidance of the people and their teachers and rulers—of that ancient pride in health as a cultural objective which has been largely in abeyance since the days of the old Greek civilisation.

The training of the doctor, which began with observations on and the care of the sick individual, is due now for a great forward stride. Observations on whole communities, whether great or small, (or on appropriate samples) and improved health provisions for them, must henceforward become the prior objective. The individual is not likely to suffer neglect in the process, for all communities are composed of individuals. For generations yet we shall doubtless continue to build our costly hospitals and clinics, and require our armies of practitioners and ancillaries, but meanwhile we must at least embark upon the crusade which will end in the steady reduction of waiting-lists and the closure of hospital wards, and which will eventually put the physical, mental and moral health of peoples before their material wealth. In that crusade —whether by our researches, by realistic reforms in teaching, by the better education of the people or direct representations to government —it is our first duty as physicians to explore and prepare the way.

I submit that we can only do this effectively by electing to pursue the study of social man in sickness and in health as assiduously as we have hitherto pursued the study of individual man in the isolation of the consulting room or the hospital bed, when health has finally passed him by. The quality of our actions and our practice and of our leadership in social reformation will depend, as in the past, on many disciplines, but not least, perhaps, upon the science whose history I have briefly sketched and whose province I have endeavoured to define.

REFERENCES

Cabot, R. C. *Social work. Essays on the meeting-ground of doctor and social worker.* Boston, Houghton Mifflin, 1919.

Chadwick, E. *Report on the sanitary conditions of the labouring population of Great Britain.* London, Clowes & Sons, 1843.

Farr, W. *Vital statistics.* London, Sanitary Institute, 1885.

Grotjahn, A. *Soziale Pathologie.* 3. ed. Berlin, Springer, 1923.

Huxley, J. *On living in a revolution.* London, Chatto & Windus, 1944.

M'Gonigle, G. C. M. and Kirby, J. *Poverty and public health.* London, V. Gollancz, 1936.

Newsholme, A. *Elements of vital statistics.* London, G. Allen & Unwin, 1923.

Richardson, H. B. *Patients have families.* New York, Commonwealth Fund, 1945.

Robinson, G. C. *The patient as a person.* New York, Commonwealth Fund, 1939.

Sand, R. *Cliniques de médecine sociale à l'Hôpital Saint-Pierre.* Brussels, J. Vromans, 1938.

Simon, J. *English sanitary institutions.* London, Cassell & Co., 1890.

Sutherland, I. Stillbirth-rate in England and Wales in relation to social influences, *Lancet*, 1946, *2*:953.

CARCINOMA OF THE LUNG: A SIX YEAR STUDY*

GEORGE HOPPIN HUMPHREYS II

Director, Surgical Service, Presbyterian Hospital
Professor of Surgery, College of Physicians and Surgeons, Columbia University

THE outlook for a patient who develops carcinoma of the lung is so miserable that many doctors consider it completely hopeless. Because until recently primary carcinoma has been thought to be a rare disease, the possibility of its presence is too often overlooked by the general practitioner. Only after a protracted period of waiting for a turn for the better which fails to occur is he forced to realize that he is dealing with something more than an inflammatory process. Too often, even when the possibility is considered, there is a tendency to procrastinate, in the conviction that, if a tumor is present, the patient is in any case doomed. This tendency is increased by the fact that these lesions frequently occur in heavy smokers whose early symptoms are masked by a long history of cough, and whose illness apparently begins with symptoms of acute infection, symptoms which respond deceptively well to sulfonamide or penicillin treatment. The discomfort of bronchoscopy is a small thing to ask of a patient who is faced with possibility of death from this disease, yet it is surprising how often the chance it affords of establishing the diagnosis early is withheld.

There is now no doubt that the incidence of bronchiogenic carcinoma is increasing. It must be considered as a real possibility whenever persistent cough, chest pain or blood tinged sputum is present, or whenever an unexplained area of density is seen by x-ray which does not clear promptly and completely. Early recognition, as in all malignant tumors, is the key to successful treatment, and it is now certain that surgical treatment can be successful, though the proportion of those cured is still tragically small. In order to gain a clear picture of the situation to guide us in knowing what to expect and to determine how to save more of these patients it is necessary to review the entire picture, rather than to present results in selected groups.

* Read 6 December 1946 before the Section of Surgery of The New York Academy of Medicine.

Vital Statistics: This record presents a review of the experience in an urban teaching hospital of twelve hundred beds, over a period of six years. The records of all patients discharged from the Presbyterian Hospital in New York with a diagnosis of bronchiogenic carcinoma between January 1, 1940 and December 31, 1945, have been reviewed. There were in all 125 patients in whom the diagnosis was made, constituting about 6 per cent of the discharges from the medical and surgical services; in 103 the diagnosis was proved by microscopic examination and in the remaining 22 it was made on evidence from the findings on x-ray, bronchoscopy and clinical course. The high proportion of male to female patients reported in other series was also present in this group, there being 109 men and 16 women, a proportion of nearly 7 to 1. The youngest patient was 34 and the oldest 79, with the usual peak in the 6th and 7th decades. Those operated upon averaged 53 years. Undifferentiated tumors, classified as carcinoma, were slightly more frequent than the better differentiated epitheliomas, but the latter were almost twice as frequent in the operable group, and all of the surviving patients had this type of tumor. The anaplastic "oat cell" tumors constituted only 11 per cent of the entire group, and only one was resectable.

Symptomatology: The symptoms most frequently encountered were cough, pain and hemoptysis, and varied widely with the site of the tumor. In several, metastases caused the first symptoms, six cases first manifesting themselves with cerebral symptoms. Ten patients had no localizing symptoms, complaining only of weakness and loss of weight; several of these were moribund on admission and the accuracy of the history is doubtful. In general the duration of symptoms before admissions was less in the group not operated upon (an average of 5 months in the 79 cases giving a reasonably accurate history) than in the operated group (6.9 months) and it was longer in the resectable cases than those which could not be resected (7.3 months against 5.5 months). This suggests that the resectable tumors are a slower growing group, and corresponds to the higher percentage of well differentiated epitheliomas among them.

Operability: Two-thirds of the group (83 patients) were not operated upon, either because they were not referred to a surgeon, because they refused operation, or because they presented evidence of metastases on admission. No patient was refused operation because of age or because of evidence of chest wall invasion or the presence of pleural fluid

TABLE I

NO OPERATION 83 Patients 66.4%						OPERATION 42 Patients 33.6%			
DIAGNOSIS PRESUMPTIVE 22 Patients 17.6%		DIAGNOSIS PROVED 61 Patients 48.8%				NO RESECTION 13 Patients 10.4%		RESECTION 29 Patients 23.2%	
BRONCHOSCOPY 15 Patients 12%	X-ray 7 Pat. 5.6%	AUTOPSY 12 Patients 9.6%	BIOPSY 49 Patients 39.2%			DIED OF DISEASE 12 Patients 9.6%	DIED OF OPERATION 10 Pat. 8%	Survived Resection 20 Patients 16%	
			METASTASIS 12 Patients 9.6%	PLEURA 10 Pat. 8%	PRIMARY TUMOR 27 Patients 21.6%			DIED OF METASTASIS 17 Patients 13.5%	A 3 2.5 %

TOTAL 125 PATIENTS

unless the latter contained masses of tumor cells. Bronchoscopic evidence of tracheal distortion or mediastinal widening was not considered a contraindication, a point of view justified by the fact that one of the patients who remains well showed marked distortion of the trachea. In several instances, however, these considerations were a factor in deciding against operation without surgical consultation. Definite tissue diagnosis was not considered essential, in fact its lack indicates a peripheral and therefore more favorable growth, as demonstrated by the fact that no positive biopsy was obtained in 22 of the 29 resected cases.

One-third of the entire group (42 patients) were operated upon, and resections were completed in over two-thirds of them (29 patients). The remaining 13 patients were found to have extension of the primary growth into the hilar mediastinum, the spine, or widespread mediastinal lymph node involvement. Local extension into the chest wall or pericardium was not considered a contraindication to resection, block resection being carried out in 8 cases.

Pneumonectomy was carried out in 17 cases. In four the lesion was considered either of doubtful malignancy or sufficiently well localized to be removed by lobectomy. Since all of these tumors recurred, it is doubtful if such a limited resection is justifiable and it has been abandoned. In 7 cases portions of the chest wall were resected with a lobe and in one of these a large portion of pericardium also; in one case pneumonectomy was combined with chest wall resection.

Results: The end result in 39 cases was not recorded, but since these patients all had advanced lesions and were failing rapidly when last seen it is assumed that they died. Twenty-six of these received only diagnostic and symptomatic treatment, thirteen received radiotherapy. Of the 44 followed up to death five received no specific therapy. The average duration of life in this group was less than two months, but this figure is influenced by the fact that half of the group (13 patients) were admitted as terminal cases. Two patients in this group survived more than a year, both having squamous cell epithelioma. It may be questioned whether these patients, both of whom were denied operation because of bronchoscopic evidence of tracheal distortion, might have been candidates for resection.

Thirty patients received radiation therapy only, and three others received radiation after thoracotomy had demonstrated that the tumor could not be removed. One of the latter patients was still alive but failing

TABLE II

	Presumptive	*Proved No Op.*	*Inoperable*	*Resected*	*Total*	*Per cent*
SEX						
Male	17	51	13	28	109	87
Female	5	10	0	1	16	13
Total	22	61	13	29	125	
	17.6%	48.8%	10.4%	23.4%		

TABLE III

Age	*Presumptive*	*No Op.*	*Exp. Only*	*Resected*	*Total*	*Per Cent*
30-39	0	3	0	0	3	2.4
40-49	3	7	6	6	22	17.6
50-59	9	22	3	15	49	39.2
60-69	8	23	4	8	43	34.4
70-79	2	6			8	6.4
Total	22	61	13	29	125	

TABLE IV

CELL TYPE	*No Operation*	*Exploration Only*	*Resected*	*Total*	*Per cent*
Oat Cell	8	3	1	12	11
Carcinoma*	33	7	10	50	49
Squamous Epithelioma	20	3	18	41	40
Total	61	13	29	103	
Per Cent	59	12.6	28.3		

* Includes Adenocarcinoma and undifferentiated Epithelioma.

rapidly, with widespread metastases when last seen fourteen months after operation, having had a full course of therapy to his primary lesion and another to spinal metastases with great symptomatic relief. The average duration of life after radiotherapy was 6.5 months which is not significantly greater than the average life of the patients (excluding terminal admissions) who received no specific therapy. It should be brought out, however, that many of those so treated did not receive adequate dosage, since the therapy was not continued if palliation of symptoms did not occur early in the course. Definite palliation was accomplished in a number of cases, and it seems probable that in these the duration of life was prolonged. No patient gained prolonged palliation or apparent cure.

Of the 42 cases operated upon 13 were not resected. One died postoperatively and the remainder died an average of 5.5 months after operation. This corresponds with the average life expectancy of the unoperated group (excluding the terminal cases) so it may be concluded that, except for the operative mortality, exploration alone does not shorten life expectancy. Nine of the 29 resected cases died in the postoperative period, a mortality of 31 per cent. This mortality may be expected to improve with time, especially since the all important immediate postoperative care of the patients in this group was hampered by the fact that it had to be carried out with a staff depleted by war. Of the twenty survivors, fourteen died of metastatic disease. One patient died of pulmonary insufficiency but was found at autopsy to have a small metastatic liver nodule. Another, who had had an oat cell tumor removed by pneumonectomy, died nearly two years later of carcinoma of the larynx. It is possible that this represented an independent neoplasm, but since both tumors were anaplastic he is grouped with those who died of metastases. One patient who was still alive when last seen, but failing rapidly, with evidence of mediastinal metastases, is also grouped with those who died of metastases. The average life span of these seventeen patients after operation is 15.8 months, a very significant increase over the approximate six months otherwise to be expected. Furthermore the symptomatic relief gained during this period is much greater than the best palliation obtained by radiation. These patients were definitely benefited by their operation even though not successfully relieved of their disease.

Three of the twenty patients who survived resection are alive and

TABLE V

Operation	*Average Age*	*Duration of Symptoms*	*Duration of Survival*	*Post-Op Deaths*		*Died of Disease*		*Apparent Cure*		*Total*	
		Months	Months	No.	%	No.	%	No.	%	No.	%
Pneumonectomy	56	6.6	10.8	7	41	7	41	3	18	17	40
Pneumonectomy Resection of Chest Wall	45	5.5	0	1	100	0	0	0	0	1	2
Lobectomy Resection of Chest Wall	57	9.5	10.6	1	14	6	86	0	0	7	17
Lobectomy	52	7	32	0	0	4	100	0	0	4	10
Exploratory Thoracotomy	52	5.8	5.5	1	8	12	92	0	0	13	31
Total	53	6.9	7.7	10	23	29	69	3	8	42	

apparently free of disease. All had squamous cell epitheliomas removed by pneumonectomy and all three are back working at their former occupations three or more years after operation. The first was a German-American sheet metal worker of 62 who had first noted a wheezing on respiration 9 months before admission. A tumor mass was seen on bronchoscopy protruding from the right middle lobe bronchus which on biopsy proved to be a squamous cell epithelioma. Following pneumonectomy he developed a small cerebral embolus and then an empyema which required open drainage and subsequent thoracoplasty. His wound has now been healed for three years and he carried out a full day's work in a shipyard during the last two years of the war. The second survivor was a Jewish furrier of 40 who had been treated for suspected tuberculosis for 10 months because of repeated hemoptysis. During this period his left main bronchus had gradually become completely obstructed with complete atelectasis of the lung and considerable mediastinal distortion. Following pneumonectomy he developed a small empyema which healed after open drainage. He returned to work six months after operation which is now nearly three years ago. The third survivor is also a furrier, a Greek of 45 who had suffered from symptoms of recurrent pneumonia, yielding to sulfonamide treatment but recurring

as soon as treatment was stopped, for four months before operation. Bronchoscopic examination showed no tumor, but the history and x-ray picture were considered sufficiently suggestive to warrant pneumonectomy, which revealed a squamous cell epithelioma of the left upper lobe. He made a smooth recovery and has been back at work since two months after operation, now four years ago.

Summary

1. A series of 125 unselected cases of primary bronchiogenic carcinoma admitted to a general hospital service over a six year period has been presented.

2. In two-thirds of the group diagnosis was made either at autopsy or so late in the course of the disease that no operation was considered justifiable.

3. Of the group of 42 patients operated upon, it was possible to carry out some form of resection in 29, with a hospital mortality of 31 per cent.

4. Only 3 of the 20 patients surviving resection remain alive without evidence of metastasis, but the lives of those who died of metastasis were prolonged and their symptoms were alleviated.

5. Radiation therapy has not increased the life expectancy in the group treated as compared to those untreated, but in many cases it has relieved symptoms and in some it has probably prolonged life.

6. Three patients are living and apparently well without physical or economic disability 3 to 4 years after pneumonectomy.

RICKETTSIALPOX*†

MORRIS GREENBERG

Acting Director, Bureau of Preventable Diseases

and

OTTAVIO PELLITTERI

Epidemiologist, New York City Department of Health

DEFINITION

RICKETTSIALPOX is the name which has been proposed for a newly discovered disease of rickettsial origin. This disease is characterized clinically by the appearance of an initial lesion somewhere on the body, usually without symptoms, followed about a week later by the onset of an acute illness of which the chief features are fever, general malaise, backache and headache. Some time between the acute onset of illness and three or four days later there appears a papulo-vesicular rash which may be present on any part of the body. The course of illness from onset of fever until recovery is about a week to ten days and from the appearance of the initial lesion until its disappearance about three or four weeks.

INVESTIGATION

Towards the end of June, 1946, the Department of Health became aware of the occurrence of cases of an undiagnosed illness in a certain section of Queens. Reports of three cases were received from Leon N. Sussman of Manhattan.[1] A number of cases were reported by Benjamin Shankman[2] and Harry N. Zeller of Kew Gardens. Joan Daly of Forest Hills gave us the reports of some cases that she had seen and Victor Stern of the Riverdale section of the Bronx was kind enough to send us his clinical records of ten cases that he had seen in one large apartment house in the past few years and which he had diagnosed as atypical chicken pox. Reports of isolated cases were received from many other physicians, to all of whom we are greatly indebted.

* From the Bureau of Preventable Diseases, New York City Department of Health.
† Presented at the Pediatric Section of The New York Academy of Medicine, December 12, 1946.

Fig. 1—Initial lesion of Rickettsialpox in interdigital space between fourth and fifth toes. (A button has been placed between the toes to bring out the lesion more clearly.)

In the beginning of July, a preliminary investigation was made in the Kew Gardens section of Queens where most of the cases were located. It soon became obvious that a complete study would require the joint efforts of clinical, epidemiological, and laboratory teams. Charles Armstrong of the National Institute of Health was requested to help in the laboratory work and Robert J. Huebner was assigned to this study. Later, William L. Jellison from the Rocky Mountain Laboratory of the National Institute of Health in Montana joined the team as Parasitologist and remained resident in New York City for a number of weeks in charge of a field laboratory which was set up. This coöperative study worked out very smoothly.

Clinical Features

A clinical description of the cases has been reported.[3] The important features are three, an initial lesion followed by an acute onset of fever and later by the development of a rash.

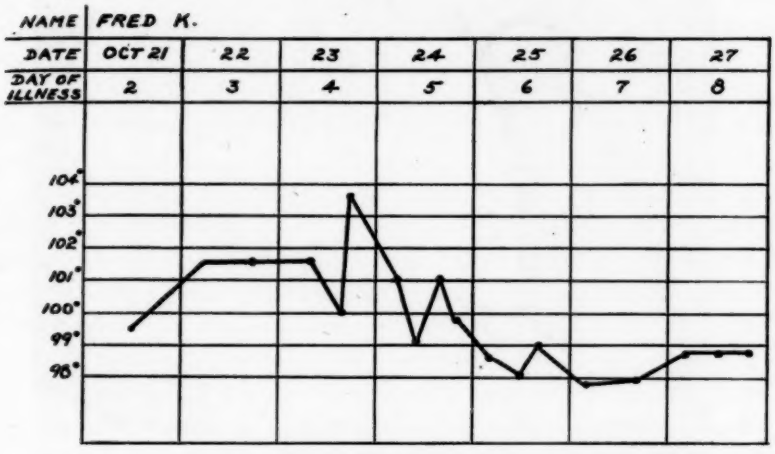
1. *Initial lesion:* This is present in most of the cases. However, since the lesion gives no symptoms and since a rash develops later, patients occasionally overlook the initial lesion and confuse it with the secondary rash. Of the patients questioned 72 per cent had noticed the lesion. However, in the cases seen and examined by us 95 per cent showed evidences of a lesion. The initial lesion usually starts as a small papule which is deep-seated and which gradually enlarges sometimes with slight erythematous reaction at the periphery. The papule may be round or

Fig. 2—Initial lesion of Rickettsialpox, lower part of right chest. The lesion is in its terminal stage, only the black eschar being present.

oval and the diameter at its base varies at the height of its development from about ½ cm. to 1½ cm. (Fig. 1). A deep-seated vesicle is always present. It is usually firm, not easily broken and the fluid in it is at first clear but may later become cloudy. The vesicle may be accidentally broken by scratching and some of the fluid exude. At other times, if the vesicle is not broken it tends to shrink and dry. A black eschar forms at the center of the lesion (Fig. 2). This persists for a period of one or two weeks when it drops off leaving a small scar. The entire evolution from beginning to end is about three to four weeks.

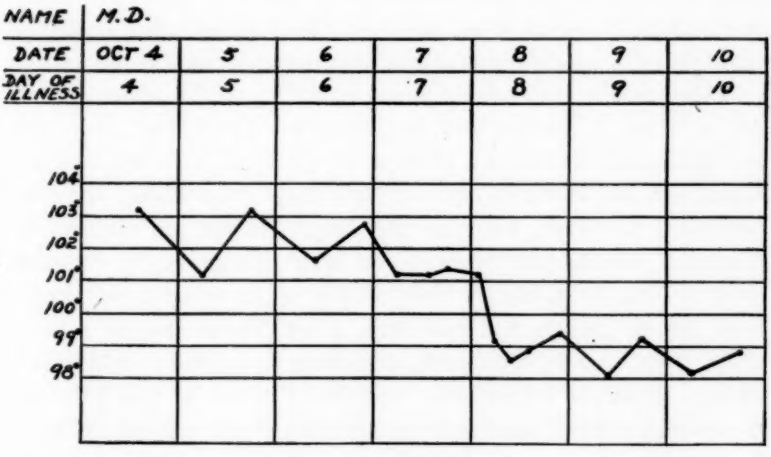
The lesion is not tender nor does it itch. There is usually an enlargement of the regional lymph glands and sometimes these are slightly tender to the touch. Lymphangitis, however, does not appear. The initial lesion may occur on any part of the body, but has been seen more often on the covered parts.

2. *Onset of the disease.* This is sudden and is characterized by chills, fever, sweats and backache. It usually occurs about a week after the



WBC—6300 L—39
P—58 M—3

Chart 1—Temperature chart of a case of Rickettsialpox.



WBC—5150 P—7 L—21

Chart 2—Temperature chart of a case of Rickettsialpox.

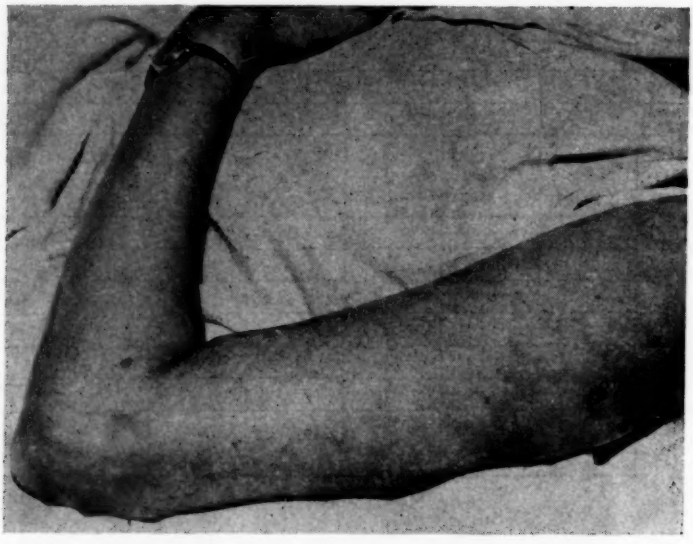


Fig. 3—Rash in a case of Rickettsialpox.

onset of the initial lesion. Fever and lassitude occurred in all the cases, headache in about 90 per cent, chills or chilly sensations, usually recurrent, in about 70 per cent, sweats in 75 per cent, backache in 50 per cent and photophobia in 40 per cent. Other symptoms complained of occasionally were anorexia, nausea, dry throat, vomiting and vertigo.

Fever usually rises rapidly to reach 103° to 104° F. about the second day of illness. It persists with remissions for a period of about a week and gradually defervesces (Charts 1, 2). *Chills* or chilly sensations occur, as a rule, in the first two or three days of the disease, occasionally preceding the fever. They usually occur, though not always, in the afternoon; occasionally patients feel well enough in the morning to want to go to work but are compelled to return home in the afternoon. Some of the patients had two chills a day. *Sweats* usually follow the chills. They may be sufficiently drenching to require a change in bedclothes. *Headache* is frequently complained of and is often quite severe. It is usually frontal; some patients complain of pain behind the eyeballs. Occasionally the complaint is of pain and stiffness in the nape of the neck. In none of the cases, however, was there any question of nuchal rigidity. *Backache* and general soreness of the muscles occur early and

with it there is always present a certain lassitude and a feeling of wanting to stay in bed. Some of the patients complain of *photophobia.* The bright light bothers them. Occasionally a mild injection of the conjunctivae is present. Infrequently there are other complaints, such as nausea and vertigo. Strangely enough anorexia is not a common symptom; patients usually eat fairly well. Nor are there other gastro-intestinal symptoms.

3. *Rash:* The third part of the clinical triad is the rash (Fig. 3). This appears in all cases and is noticed most commonly by the patients between the day of onset of fever and four days later. In those cases that we personally followed, the rash appeared to come on some time after onset of fever. However, many of the patients do not take to bed and do not take their temperature until they have been sick a day or two and they report the rash as occurring on the day of onset. In 90 per cent of the cases investigated, the rash occurred between the day of onset of fever and four days later. The lesions are discrete, erythematous, maculo-papular in character. Usually the papules are either rounded or flat but occasionally appear to be conical in shape. They are always firm to the touch. Surrounding the roughly circular base there is occasionally slight erythema of the skin. A vesicle develops at the summit of the papule in most of the cases. This is firm and dries after a few days, forming a black crust which falls off after another few days without leaving any scar formation. The rash is usually sparse. There is always normal skin between the individual lesions. However, occasionally it is more widely distributed and sometimes almost generalized over the body. No particular sequence of occurrence is noted. In some cases the rash appears first on the face or chest, in others on the back or abdomen, and in still others on the arms or legs. It was seen on the mucous membranes in only two cases, once on the palate and once on the tip of the tongue. It was not seen on the palms of the hands or the soles of the feet. The lesions vary in diameter from about 2 mm. to 8 mm. As with the initial lesion there are no subjective symptoms accompanying the rash. The duration is approximately a week, although in mild cases it lasts not more than two or three days while in severe cases it lasts as long as ten days. As a rule a brownish discoloration remains for a few days after the rash disappears.

In spite of the severity of the acute symptoms and the height of the fever and the discomfort of the chills and sweats, the patients do not

appear seriously ill. There is no evidences of dehydration, no stubborn refusal of food or drink; the pulse is usually good and respirations are normal. In a few cases the spleen becomes enlarged for a short period.

Laboratory Findings

Normal red counts and hemoglobin determinations were found in the cases. White blood counts showed a moderate leukopenia in practically all cases where they were done, varying between 2400 and 7500 white blood cells per c. mm. The differential smear was either normal or showed a relative lymphocytosis. The leukopenia lasted during the acute illness and the blood returned to normal usually within a week or two after recovery. Urine examinations were normal except for a transient febrile albuminuria in a few cases. Blood cultures were done early in the disease in many of the patients and were repeated in some. All were negative. Sedimentation rates were normal or showed slight elevation. Agglutination tests were performed with blood sera from patients in the acute and in the convalescent stages. They were tested for the typhoid, paratyphoid groups, brucella, Leptospira icterohemorrhagiae, Leptospira canicola, tularemia and heterophile antibody. These were all negative. In a few cases complement fixation tests were performed for psittacosis. These were also negative.

Agglutination tests were done with the sera of a considerable number of recovered patients for B. proteus OX 19, OX 2, and OX K. With exception of a few that agglutinated in low titres, all were negative (Table 1).

Complement fixation tests were performed for epidemic typhus, murine typhus, tsutsugamushi fever, Colorado tick fever and Q fever. These were all negative.*

Pathology

Biopsies were made of a primary lesion, of skin lesions, and of a lymph node. The pathological picture is reported as similar to that found in lesions of other rickettsial diseases.

Etiology

Blood speciments were obtained from many of the patients during the acute stage of their disease and inoculated into a variety of labora-

* We are indebted to Dr. Herald L. Cox of the Lederle Laboratories for performing some of these tests for us.

TABLE I—RESULTS OF AGGLUTINATION TESTS OF SERA FROM PATIENTS OF RICKETTSIALPOX WITH B. PROTEUS ANTIGENS

Patient	*Date of Onset*	*Date of Specimen*	*OX 19*	*OX 2*	*OX K*
Mrs. C.	3/15/46	7/24/46	neg.	neg.	neg.
Mr. C.	3/29/46	7/24/46	neg.	neg.	neg.
D. M.	5/30/46	9/17/46	neg.	neg.	1:20
M. G.	5/31/46	9/ 7/46	1:20	1:10	1:10
B. B.	6/ 9/46	7/11/46	neg.	neg.	neg.
Mr. S.	7/ 7/46	8/ 8/46	neg.	neg.	neg.
C. F.	7/11/46	9/13/46	neg.	1:20	1:20
M. B.	7/15/46	7/26/46	1:10	neg.	neg.
M. S.	7/17/46	7/24/46	neg.	neg.	neg.
		8/18/46	1:10	1:10	neg.
C. B.	7/17/46	8/29/46	neg.	neg.	neg.
J. M.	7/18/46	8/18/46	1:40	neg.	neg.
L. A.	7/18/46	7/24/46	neg.	neg.	neg.
		8/18/46	neg.	neg.	neg.
M. K.	7/22/46	8/29/46	neg.	neg.	neg.
L. A.	7/22/46	8/18/46	neg.	neg.	neg.
Mrs. S.	7/25/46	8/18/46	neg.	neg.	neg.
H. B.	7/27/46	8/18/46	neg.	neg.	neg.
M. M.	8/10/46	8/30/46	neg.	neg.	neg.
M. A.	8/18/46	8/26/46	neg.	neg.	neg.
		9/11/46	1:40	1:10	neg.
I. M.	8/20/46	8/30/46	1:20	1:10	neg.
M. S.	9/ 6/46	9/13/46	1:10	1:10	neg.
		10/ 9/46	1:10	1:40	1:10

tory animals. An organism possessing the characteristics of a rickettsia was recovered from the tissues of a mouse, inoculated with blood drawn from one of the patients (M.K. strain). The isolation of this organism has already been described.[4] The name Rickettsia akari has been proposed for it.[5]

A second strain of Rickettsialpox was isolated from the blood of another patients (M.S. strain). The latter is culturally and immunologically similar to the M.K. strain. The M.K. organism grows abundantly in the yolk sac of embryonated eggs (Fig. 4). Antigens prepared from this organism were tested by complement fixation with the sera of recovered cases of Rickettsialpox as well as the sera of normal human beings, of recovered cases of syphilis, and of some of the rickettsial diseases, such as endemic typhus, tsutsugamushi fever, Q fever and Rocky Mountain spotted fever. Excepting for cross reactions with Rocky

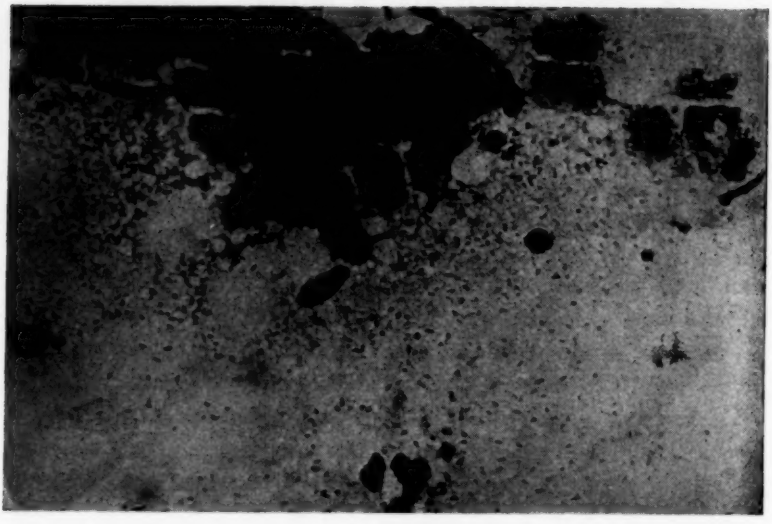


Fig. 4—Smear of Rickettsia akari from yolk sac. Stained by Machiavello's method.

Mountain spotted fever, a high degree of specificity for recovered cases of Rickettsialpox was shown.[4] Table II lists some recovered cases of Rickettsialpox. It will be noted that the sera of all were positive by complement fixation for the M. K. antigen. It will also be noted that there is usually cross agglutination with Rocky Mountain spotted fever but as a rule in lower dilutions.

Differential Diagnosis

1. *Other Rickettsial Diseases.* (a.) *Rocky Mountain spotted fever:* In this disease there is a history of a tick bite in many of the cases. The disease itself is much more severe and mortality is frequently high. The rash is more generalized, often petechial, leukopenia is not characteristic and there is no initial lesion. Furthermore, most cases of Rocky Mountain spotted fever give a positive agglutination reaction for Proteus OX 19 or OX 2.

(b.) *Epidemic typhus.* The rash in this disease is similar to that of Rocky Mountain spotted fever. No initial lesion is seen. The patients are usually much sicker than in Rickettsialpox and mortality is usually high. Agglutination of the patient's serum with B. proteus OX 19 is

TABLE II—COMPLEMENT FIXATION TESTS OF SERA OF PATIENTS TESTED WITH THE M. K. STRAIN OF RICKETTSIALPOX AND WITH ROCKY MOUNTAIN SPOTTED FEVER ANTIGEN

Name	*Date of Onset*	*Date of Specimen*	*Titers with* *Rickettsial Antigen (M. K. Strain)*	*Titers with* *Rocky Mountain Spotted Fever Antigen*
F. M.	5/30/46	9/17/46	1:64	1:16
G. G.	6/25/46	9/19/46	1:32	1:16
W. V.	7/ 5/46	9/11/46	1:64	1:8
R. B.	7/ 8/46	9/30/46	1:16	neg.
J. B.	7/14/46	9/21/46	1:64	neg.
J. D.	7/14/46	9/22/46	1:128	1:64
C. B.	7/19/46	8/29/46	1:64	1:8
C. C.	7/20/46	9/23/46	1:32	1:8
A. G.	7/23/46	9/19/46	1:128	neg.
N. S.	7/30/46	9/24/46	1:128	1:64
F. G.	8/ 1/46	9/22/46	1:32	1:16
E. G.	8/ 2/46	9/19/46	1:16	neg.
J. K.	8/ 8/46	9/22/46	1:64	1:8
L. H.	8/10/46	9/24/46	1:32	1:8
D. G.	8/16/46	10/23/46	1:32	1:8
N. Sh.	8/17/46	9/23/46	1:64	neg.
I. M.	8/20/46	8/30/46	1:32	1:8
		9/11/46	1:32	1:8
E. K.	9/3/46	9/ 7/46	neg.	neg.
		9/11/46	1:4	neg.
M. S.	9/ 6/46	9/13/46	neg.	neg.
		10/ 9/46	1:512	1:512
A. R.	9/22/46	10/ 8/46	1:512	1:256
W. R.	10/21/46	10/30/46	1:4	neg.
		11/ 5/46	1:512	1:256

almost always positive. Furthermore, there is usually a history of lice infestation.

(c.) *Endemic typhus:* While this disease is comparatively mild the rash is macular or slightly papular but not vesicular and usually is generalized following a centrifugal distribution. No primary lesion is seen. Agglutination of the patient's serum with Proteus OX 19 is the rule.

(d.) *Tsutsugamushi fever or scrub typhus:* Although an initial lesion is present in many cases of Tsutsugamushi fever, the rash resembles that of typhus fever rather than Rickettsialpox and it is not vesicular. The course of the disease is more severe and respiratory symptoms occur.

The patient's serum agglutinates with B. proteus OX K usually about the second week.

(e.) *Boutonneuse fever:* The resemblance of Rickettsialpox to Boutonneuse fever is closer than to that of any of the other rickettsial diseases. Boutonneuse fever is also comparatively mild and is initiated by a primary lesion similar to that in Rickettsialpox. However, the rash in Boutonneuse fever is papular or maculo-papular and frequently involves the palms of the hands and the soles of the feet. Furthermore, agglutination of the serum of patients with B. proteus OX 19 occurs during the convalescent stage. Also, there is usually a history of association with dogs and of a tick bite.*

2. *Chicken pox:* The vesicles are totally different in the two diseases. In chicken pox the entire papule becomes vesicular so that at certain stages only a vesicular rash is seen. The vesicle is thin and easily broken. In Rickettsialpox the vesicle always surmounts the papule and is firm. Furthermore, there is no initial lesion in chicken pox and the fever does not, as a rule, occur before the onset of the rash. In Rickettsialpox adults as well as children are affected and many of the patients give a history of previous chicken pox.

3. *Smallpox:* Smallpox is a much more severe and serious disease than Rickettsialpox. The rash usually appears on the hands and feet as well as on the face, the maturation of the pock is totally different than in Rickettsialpox and the pocks heal with scar formation.

4. *Infectious mononucleosis:* The occasional occurrence of a rash in infectious mononucleosis and the occasional lymphadenitis and enlarged spleen in Rickettsialpox might tend to confusion in diagnosis. However, the blood picture in infectious mononucleosis is characteristic and the heterophile reaction is usually positive. Furthermore, the triad of initial lesion, acute onset with fever, and rash does not occur in infectious mononucleosis.

Epidemiology

The largest number of cases and those first seen by us all lived in an apartment project consisting of three groups of three-story apartment buildings occupying three square city blocks in Queens.[6] The area is residential, about 15 miles from the center of the city and the houses

* Professor J. Caminopetros of the Hellenic Pasteur Institute at Athens, who has seen many cases and written extensively on Boutonneuse fever saw some of our patients while on a visit in this country during October, 1946. He stated definitely that the two diseases were different.

were built about six years ago. The general locality is suburban in type with wide avenues on which there is considerable traffic from New York to the outlying parts of Long Island and back. The streets are paved, the sewage system is modern in type and part of the sewage system of New York City. Water is supplied from the city's regular water supply by means of underground pipes.

A preliminary survey soon indicated that neither age nor sex was a factor in the occurrence of cases. Water supply was quickly ruled out and so was the milk supply which was all pasteurized and which was used by other inhabitants of Queens, and furthermore came from several sources. Other food supplies were likewise eliminated as possible sources of infection. Groceries, baked foods and meats were purchased from a number of different markets and the same stores catered also to other residents of the neighborhood. Although individuals living close to each other were acquainted, there were no common meetings, nor did they visit with each other.

The physicians in the neighborhood informed us they had seen no cases in other houses in the vicinity although their practices were not limited to this housing development.The source of infection appeared to be in the houses themselves.

The apartments were occupied largely by middle class, married young people. They were usually of three or four rooms and most of those that we visited were clean and neat. The tenants did not complain of insects, many had screens on their windows but even those that did not, stated that except for occasional flies and some small fleas they were not bothered by insects. Mosquitoes were uncommon and ticks were not seen. All the tenants, however, complained of mice in the houses. Some had seen them in their own apartments; others had seen them chiefly in the basement and storage rooms of the house and in and around the courts. It was decided to set traps in the various buildings and in and around the courts so as to catch the mice and comb them for ectoparasites. The discovery by Mr. Charles Pomerantz, of mites on the basement walls facilitated the investigation.* A systematic search of all the buildings in the group was undertaken. A laboratory was set up in the basement of one of the buildings and William L. Jellison, Parasitologist, from the Rocky Mountain Laboratory of the Division of

* The specimens were identified by Dr. E. W. Baker of the Bureau of Entomology and Plant Quarantine, U. S. Department of Agriculture as Allodermanyssus sanguineus (Hirst).

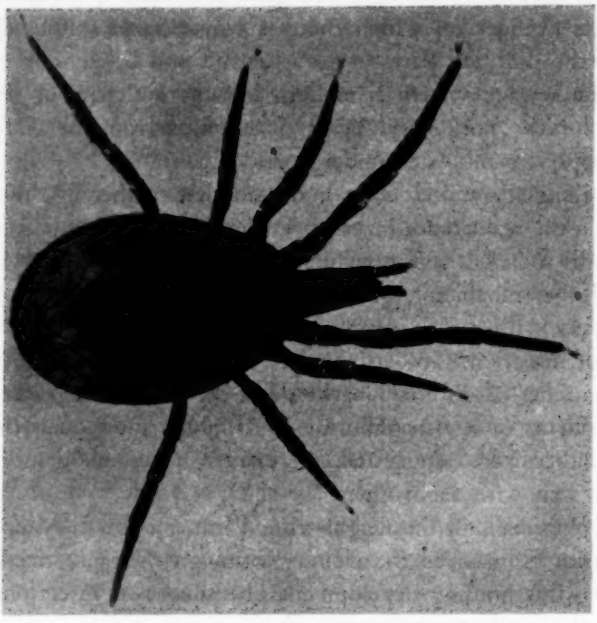


Fig. 5—Allodermanyssus sanguineus (Hirst) adult stage.

Infectious Diseases, National Institute of Health, was put in charge.

Mites were recovered from the storage rooms and basements of most of the buildings (Fig. 5). They were found chiefly on the walls, particularly near incinerators. They were also found as ectoparasites of freshly trapped mice (Mus musculus) in the buildings. Some of the mites were flat but many were engorged, indicating that they had recently fed. Smears made from these showed mammalian erythrocytes. From two pools of these rodent mites rickettsiae were isolated. The strains were apparently identical with the rickettsia which had been isolated from the blood of two of the patients.[5]

Comment

Although the original cases were all reported from one particular housing development in Queens, many other individual and groups of cases have been reported in other boroughs. The investigation of these cases is proceeding. In at least one house in the Bronx where a number of cases of Rickettsialpox had occurred, similar mites (A. sanguineus)

were also found on the walls of the basement, and mice were abundant.

The etiology has been definitely established by the isolation of rickettsiae from the blood of patients and from the ectoparasites of mice. The fact that these mites are found in buildings where mice are always present suggests that the mice act as definitive hosts of the mites and that humans are probably infected when they come in contact with and are bitten by infected mites. Control measures will probably depend on methods of elimination of mice and mice harborages from apartments in which they are present.

Summary

The picture of a new disease caused by a hitherto undescribed rickettsia has been given. The main clinical characteristic is the occurrence of a triad consisting of an initial lesion, followed a week later by the acute onset of fever, headache, chills, sweats and backache, and thereafter by the occurrence of a papulo-vesicular rash two to three days after the onset of fever. The duration of illness from onset of initial lesion is about three to four weeks. During the acute illness there is a moderate leukopenia and the sedimentation rate is either normal or slightly elevated. The sera of recovered cases give negative agglutination reactions with B. proteus OX 19, OX 2 and OX K, and give positive complement fixation tests for Rickettsialpox, using the M.K. strain as antigen. The causative agent of the disease is Rickettsia akari and the vector is a rodent mite, Allodermanyssus sanguineus.

There have been no fatalities from the disease.

REFERENCES

1. Sussman, Leon N. Kew Gardens' spotted fever, *New York Med.*, 1946 *2*:27-28.
2. Shankman, Benjamin. Report on an outbreak of epidemic febrile illness, not yet identified, occurring in New York City, *New York State J. Med.*, 1946, *46*:2156-2159.
3. Greenberg, Morris, Pellitteri, Ottavio, Klein, Irving F., and Huebner, Robert J. Rickettsialpox—a newly recognized rickettsial disease. II. Clinical findings *J.A.M.A.*, 1947, *133*:901-906.
4. Huebner, Robert J., Stamp, Peggy, and Armstrong, Charles. Rickettsialpox—a newly recognized rickettsial disease. I. Isolation of the etiological agent, *Public Health Reports*, 1946, *61*:1605-1614.
5. Huebner, Robert J., Jellison, William L., and Pomerantz, Charles. Rickettsialpox—a newly recognized rickettsial disease. IV. Isolation of a rickettsia apparently identical with the causative agent of Rickettsialpox from Allodermanyssus sanguineus, a rodent mite, *Public Health Reports*, 1946, *61*:1677-1682.
6. Greenberg, Morris, Pellitteri, Ottavio, and Jellison, William L. Rickettsialpox—a newly recognized rickettsial disease. III. Epidemiology *(in press)*.

PLATO AND CLEMENTINE*

HOMER W. SMITH

New York University College of Medicine

I HAVE been thinking much, of late, about the word 'normal'—the physiologically normal, the anatomically normal—a word which we use a dozen times a day. Webster defines the normal as that which conforms with natural law, but by this definition only the supernatural is abnormal. This distinction is not very useful to a physiologist, so I have been trying to discover what *I* mean by normal. I am afraid that for all my thinking I have come out by the same door where in I went, but while I was wandering through this labyrinth of thought I have come upon some collateral matters that have perhaps left the experience something better than an absolute loss. I invite you to join me in retracing my labyrinth for whatever the adventure may be worth.

Our use of the word 'normal' in biology and its derivative sciences stems, I think, from the Greek philosopher, Plato, whose name I would put high in the list of the intellectual enemies of mankind, among those personages who should be starred with black daggers in the book of history for falsely directing, and thus retarding, the development of human thought. Historians have long since charged Plato with economic and political bias in the development of his philosophy; they have traced to his Idealism some of the major elements in late Roman and Medieval times which strangled intellectual inquiry and fostered the spiritual development and mental stagnation of the Dark Ages; they have pointed to the Renaissance and the succeeding centuries, when modern science was born and developed, as a period during which Platonism was driven out of one field after another. They assume that today we are reasonably free of its philosophic aberrations. In this last I think they are in error. Determinism applies as much to the shaping of human thought as to the shaping of mountains, and we may have to wait until a new intellectual age when the mountains which we now

* Morris Herzstein Lecture (III) delivered at the University of California, December 6, 1946. The first of these lectures appeared in the April 1947 issue of the Bulletin.

know are worn away before we shall be wholly free of those distortions in our thoughts which are traceable to this Athenian aristocrat.

Determinism applies also to the making of men, and Plato was a product of his environment, of local circumstances, and of his innate constitution. "I thank God," he said, "that I was born a Greek and not a barbarian, freeman and not slave, man and not woman; but above all, that I was born in the Age of Socrates." Here are the keynotes to his philosophy, the philosophy of an aristocrat, a snob, a bigot, and of a man with limited perspective.

Long before his time the land of Greece had suffered the troublous transition from a primitive tribal culture, characterized by inherited family privileges, to a loose conglomeration of city-states grown rich with vulgar commerce and ever seeking new and more profitable adventures. Trade had produced great personal wealth and this wealth had sought safety in reinvestment in the land until private property had become the foundation of social distinction, of the new oligarchy of rich and poor, aristocrats and commoners, freemen and slaves. It was in this turmoil between the old and the new that Socrates was hailed to trial by the democrats and condemned to death on the charge that he had been aiding and abetting the aristocratic Thirty.

Plato, born an aristocrat, hated the democrats before Socrates' death, and after this tragedy he was fired with bitterness and driven into philosophy as the only politically safe way in which to pay off the vulgar horde. He thereafter devoted himself to the discovery of that special kind of "truth" or "justice"—the two were essentially synonymous in an age when the only standard of reference was politics—which would protect aristocratic interests. Truth for Plato was identified with tradition, with inherited lands and privileges, with stability and unchangeableness, with divine decree that transcended the individual; truth was the antithesis of individuality as well as of change, of all the hated democratic forces that worked like ferments in the mob of sailors, shipowners, traders, money-lenders, wheat speculators, adventurers, slaves, to keep them in a perpetual state of turbulence and revolution. Democracy not only placed the individual on a pedestal but it defended the very rightness of change as a natural and just phenomenon. Plato despised democracy because in fact and in principle it threatened the security of his aristocratic privileges, and in the end he rhetorically annihilated it, and all possibility of its existence, by philosophic decree.

He argued that since change is the denial of the unchangeable, the absolute, and since only the unchanging and absolute can be either knowable or true, that which changes is untrue, a half-truth, an illusion of the senses.

The *physiologi*, Thales, Anaxagoras, Heraclitus, Democritus, had asserted that what truly exists is matter which contains within itself all the laws necessary to its existence and operation, and all else is but flux and movement of atoms. These persons Plato classed with the damnable democrats, the defenders of flux, under the most scathing term he could think of—*materialists* who ignorantly took as real the objects of the world as they appeared to be, and falsely presumed to find in change and process something that is both knowable and significant. Thus developed his philosophy—since what is true must always be true, truth is absolute and eternal; the unchanging and unchangeable is the only reality, and all that deviates from it is the product of error.

Thus he built his philosophy of the Ideal with the aid of the Pythagorean theory that only numbers are real, and with the aid of the Pythagorean term, Idea, or the essence of things. To cite the oft-quoted example, a particular circle is drawn by hand and proves to be slightly eccentric, so it is erased and another circle is drawn, and another, and still another, no one of them ever perfect, no one of them indestructible, no one of them to last forever. But the archetype of the circle, call it perfect circularity if you will, was there before the hand attempted the first imperfect circle and will survive after the last imperfect circle has been erased. It alone is real. And so it is that all particular objects of whatever kind approach in greater or lesser degree some Ideal or universal. The Ideal circle is not inherent in the substantive circle which we draw, but in the mind; but not in my mind or yours, or of any particular man, for particular men come and go; since the Ideal circle endures everywhere and forever, it must be inherent in some universal, unchanging mind, which can only be the mind of God.

The world, which to our naive view appears to be made of substantive things, is, according to Plato's argument, but a shadow-show of half-truths: its particular circles, its particular men and cities and laws, indeed its particular apples and catfish and billygoats, come and go, all of them but imperfect images of the perfect and eternal Ideas of the divine mind. The Ideal billygoat (or its mathematical counterpart) existed in the divine mind before the first actual billygoat was, or smelled,

and will continue to exist and smell when the last actual billygoat, imperfect in form and odor, has ceased to be. If the democrats, the defenders of the individual, of change, of conflict, of revolution, thought for one moment that they, too, might be an Ideal in the mind of Zeus, an ultimate to which they approximated however imperfectly, they stood convicted of a vulgar and unphilosophic error—nothing that suffered change or conflict or revolution could exist in the divine mind and therefore it could not be true.

It has been said of Aristotle, Plato's pupil, that he loved his master but that he loved truth more. He forthrightly denied Plato's doctrine of the Ideal. He believed that reality consisted of particular things, and not of universals; he accepted that change and process were as much a part of nature as fixity, and indeed he doubted the existence of the absolute. The world of philosophy has ever since been divided between Plato and Aristotle, and indeed Coleridge said that men are born either Platonists or Aristotelians. From the days of the Academy philosophers have continued to wrestle with the opposition between the Platonic universal, representing what we may call the philosophic norm, and the Aristotelian thing as is. The Christian church inherited Platonism largely through Neo-Platonism, and biology inherited Platonism largely through the Christian church.

Platonic Idealism intrudes itself into our thinking whenever we use the term "normal," since the word implies both perfection and predetermined plan. It also implies some measure of stability, for a standard of reference ceases to have meaning if it is never twice the same. It implies a pattern or paradigm which exists apart from things themselves, since we sense that perfection is not to be found in any one individual but can only be approximated to by abstraction from a relatively large group.

Biology has both historic and factual warrant for conceiving the normal as uniquely stable. It was recognized long before Aristotle that in each species of animals and plants one can perceive a type which is reproduced generation after generation. Indeed, our grandfathers adhered so strongly to this doctrine that they believed that God had created all animal and vegetable species according to type in the year 4004 B.C., after which Adam had named them, and that they had remained unvaryingly true to type ever since. Post-Darwinian biologists recognize that Bishop Usher was wrong in his calculation and that our

grandfathers were wrong in their faith, that specific types had actually come into existence hundreds of thousands, some of them hundreds of millions of years ago. Yet the extension of geologic ages serves only to emphasize that this stability of type is one of the most emphatic facts in nature—it is change in type that is so infrequent as to seem miraculous. Certainly we are tempted to believe that nature has a template upon which she patterns every starfish, every oyster, every man, and it is in large measure this stability which imbues us, as it imbued Plato, with a strong sense of the absolute in our use of the word "normal."

Yet we perceive that within each species individuals are not all identical, but deviate above and below a mean in every character. We point to the influences of environment on development. or to the chromosomal mechanism of reproduction, and infer that during reproduction or development this or that little accident occurred, and consequently one little starfish is a little longer in the legs than another. The "normal" starfish would be one escaping all such accidents, it would perhaps represent the Ideal template.

It required the *Origin of Species* to liberate biology from the absolute, the unchanging, which was the essence of Platonic-Christian theology. The thesis of the *Origin* was biological variation, for it was on the observed data of biological variation that Darwin laid the foundations of evolution, and Mendel the foundation of genetics. Biological variation is the antithesis of both the Platonic Ideal and the stable Norm since a variable absolute is by definition a contradiction of terms—nonsense. Yet we still quite generally fail to recognize this fact. Striving for specious simplicity we tend to exclude variation from our thinking, either by ignoring its existence or by mathematically compressing it into a rigid, artificial mold. We tabulate the anatomical and physiological dimensions of our fellow mortals and, after averaging them, we correct them to what we call "ideal man," never blushing that we know nothing about "ideal man" except that he has a body surface of 1.73 square meters and an ideal basal metabolic rate of some 40 calories per square meter per hour. We measure his cardiac output, his respiration, many functions of his glands and kidneys, and even of his psyche, and in our tables we presumptiously apply a correction factor to women to bring them up to the male's ideal measurements, without asking ourselves, Whence comes this creature of statistical perfection, this paradigm, this prototype, to whom we approximate real men (and women) by an

easy mathematical correction?

It is clear that the attributes of "ideal man" are but a collection of mathematical averages of various features which are subject to biological variation. An average value is not only a most useful one, but almost necessary to our thinking. It presents us a single concept, easily pictured and easily remembered, which we can convey to others or otherwise shape into a useful ideological tool. We could scarcely form any clear conception as to the duration of human life from observing a hundred thousand individuals, unless by taking the average; or form any idea of their adult height, weight or other vital statistics unless we reduce the pertinent data to convenient categories and thus learn what averages and ranges are to be expected. It has been said that the whole of statistics depends upon the use of such averages.

There is, of course, the other application of averages that is equally important: ten authorities estimate the magnitude of some measurable matter, or one authority makes ten estimates, and many or all of the results differ slightly from each other; if we have no reason to attach greater weight to one result than to another, experience shows us that the average of all the estimates is more likely to be right than any other value. In this usage, the average of mensuration, the mean represents a thing actually existing, and the more carefully the mean is derived the closer our concept comes to the reality; whereas in the first usage, the average of biological variation, the mean represents something that is non-existent, a mere product of our imagination, and the more carefully it is derived the more removed it is from reality, in that there is less probability of its being identified with any one individual. And for us as Aristotelians it is the individual that counts.

If we measure a large number of individuals in a given population, we find that the variations in this or that feature tend to distribute themselves above and below the mean, between certain minimal and maximal limits, in a characteristic manner. We conventionally divide the variable into equal categories and count the number of individuals in each category: when the frequency in each category is plotted against the size of the category we obtain what is called a frequency distribution curve which, in the "normal" form, is a symmetrical structure of some beauty (Figure 1). This frequency distribution curve reveals at a glance the dispersion of the variable under consideration and affords us a description of the entire population, and thus takes cognizance of

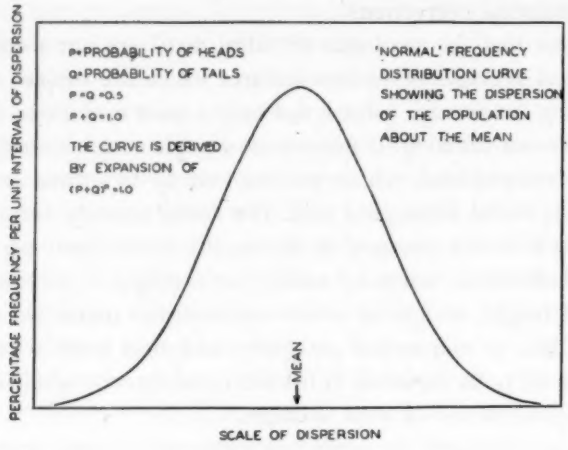

P=PROBABILITY OF HEADS
Q=PROBABILITY OF TAILS
P=Q=0.5
P+Q=1.0
THE CURVE IS DERIVED BY EXPANSION OF
(P+Q)²=1.0
'NORMAL' FREQUENCY DISTRIBUTION CURVE SHOWING THE DISPERSION OF THE POPULATION ABOUT THE MEAN
MEAN
PERCENTAGE FREQUENCY PER UNIT INTERVAL OF DISPERSION
SCALE OF DISPERSION

Fig. 1

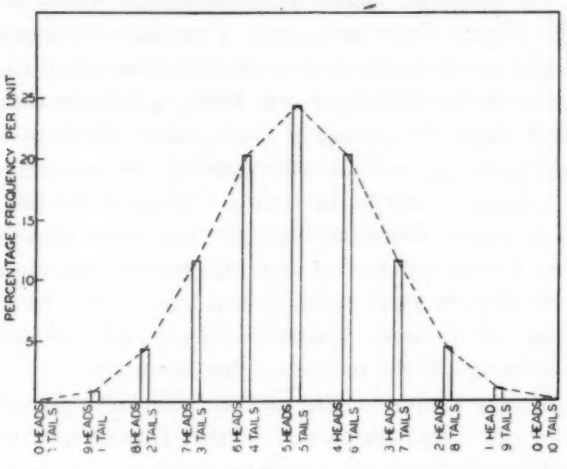


Fig. 2.

biological variation as a fact.

Because of the way our textbooks are written, this frequency distribution curve is often associated in the student's mind with errors of measurement—the terms probable error and standard error, and the use of the standard deviation, σ, as an index of the magnitude of error, constantly drives home this application. If any of us were to set a student to measuring the height of one of his colleagues and his error extended

over the range of 4 feet 9 inches to 6 feet 3 inches, we would at once stop all measurement. The term error early became attached to the curve in connection with the development of the theory of error in measurement, and here it has stuck to the misfortune of the student of biology who is thereby befuddled as to the curve's more important meaning. He fails to see it for what it is, a graphical epitome of nature, a cabalistic symbol of her compounded mysteries.

The curve presented in this figure is the so-called "normal" frequency distribution curve derived from the p:q formula applicable to the tossing of pennies for heads or tails. I need not labor you with the elementary facts that a penny has only two faces, heads or tails; that it is practically impossible for it to come to rest on its edge after descending on a hard surface, and that, assuming symmetry, the chance of its coming up heads or *p*, is precisely equal to the chance of its coming up tails or *q*, i. e., $p = q$. This equation merely says that we know of no natural or supernatural force that will influence pennies in midair in the way of favoring heads or tails; and that we can discover nothing of that kind for the simple reason that the equation always works when we test it exhaustively. For all we know there may be all sorts of gods and demons struggling amongst themselves to get heads or tails, but if we toss the penny often enough we end up with the fiends evenly matched. Or perhaps because the mass of a penny is so great, these supernatural beings, currently allowed to operate only through the Heisenberg principle of uncertainty, are unable to turn it in its fall. Anyway, we learn from experience that $p = q$, and, of course, $p + q = 1$, denoting that heads plus tails equals all the probabilities there are, unless a penny rolls off the table and gets lost, when $p + q = 0$. Were we tossing pennies so constructed that if a particular penny fell heads, one or more other pennies would as a consequence have to fall heads, pure chance would no longer operate and the frequency distribution curve could have almost any and every shape.

From these two equations it is simply a matter of algebra to derive the unsmoothed frequency distribution curve (Figure 2) showing the chances of getting any combination of heads or tails when ten pennies are thrown simultaneously and in such a manner that they will not influence each other's destiny. The symmetry of the curve stems from the fact that no one event (i.e., heads in a particular penny) influences or is influenced by any other event (i.e., heads or tails in any other

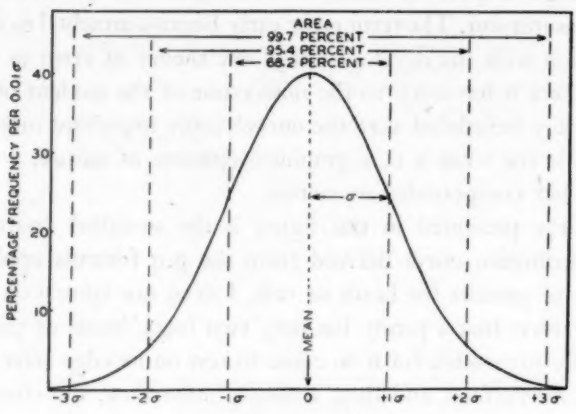


Fig. 3

penny). Hence the curve is also called the curve of independent variation, i.e., each penny comes up heads or tails independently of every other penny. The destiny of every penny is a matter of "pure chance." Why the theory-of-probability experts call the "pure chance" curve the "normal" curve is obscure, unless it is to absolve penny-tossing of any implication of demonic influence.

In the normal or theoretic curve (Figure 3), the mean, the mode and the median coincide, and the range theoretically extends from zero to infinity. In practice, frequency distribution curves rarely achieve the perfect symmetry of the normal curve and the range always extends between some minimal and some maximal finite value. The curve may, however, be narrow or wide. The normal curve has its own internal measuring rod: this is the distance from the mean to the point of inflection of the ascending or descending limbs, which affords a convenient measure of the median range, or the degree of dispersion, of the variable under consideration. This distance is well known to you under the name of σ. The structure of the curve is such that 1σ to either side of the mean will include 68.2 per cent of the population, $\pm 2\sigma$, 95.4 per cent, $\pm 3\sigma$, 99.7 per cent.

Where is the normal in relation to this curve? We cannot identify the normal with the mean for the mean is a mere mathematical abstraction not possessed in any sense by any individual in the population. Nor can we identify the mode as normal merely because the population

density is greatest at that point, nor yet the median, merely because this point stands in the middle counting from left to right. We may arbitrarily assign the normal to a band encompassing a substantial fraction of the population above and below the mean, but shall we draw this band at $\pm 1\sigma$ from the mean, to include 68.2 per cent of the population, at $\pm 2\sigma$ to include 95.4 per cent, or at $\pm 3\sigma$ to include 99.7 per cent? Clearly nothing about the curve itself will aid us in making this decision. All we can say is that at $\pm 1\sigma$, $\pm 2\sigma$, $\pm 3\sigma$ from the mean, the chances are 1 in 3, or 1 in 20, or 1 in 300, that a given individual behaves consistently with the presumptively homogeneous population from which our curve in the first instance is derived. In erecting the curve we assume that all the individuals whom we tally therein are "normal," in the same sense that we assume that all 10 of our pennies are normal. If the term normal is used at all in reference to the normal frequency distribution curve, it must include the entire range from the lowest to the highest value, the entire population must be considered normal. This definition of normal is admittedly not very useful.

But, you say, this curve is as much a mathematical abstraction as the mean itself; it differs from the mean only by having two dimensions, magnitude and frequency, instead of the single dimension, magnitude. And where is the *thing* in itself, the individual, which Thales, Anaxagoras, Heraclitus, Democritus and Aristotle, accepted as the proper subject of philosophic and scientific study? With this criticism I agree. As Aristotelians we must base our thinking upon actual and material things with three dimensions. For that reason I beg permission to include in this discussion one who conceived as flesh and blood will stand in vivid and realistic contrast to such abstractions as the population, the mean, median, and standard deviation, one who by her actual dimensions as by her admirable qualities and pleasing personality will impress us with the importance of the individual. I refer, of course, to our old friend, Clementine, whom we all recall from early, happy days.

There are some who, because of the almost pernicious accident of rhyme between her name and the size of her shoes, will remember her first on this account, but there are others, I am sure, whose minds are less inclined towards sardonic trivialities, who will recall that it was in the beautiful state of California,

In a cavern, in a canyon, *Dwelt a miner, forty-niner*
Excavating for a mine *And his daughter, Clementine.*

As Aristotelians we have agreed that it is the substantive things of life, the specific objects with mass and size and color, particular persons whom we know, that supply the proper data for scientific inquiry and philosophy. But biological variation is also a fact, and without the background of a frequency distribution curve a single individual is only an unrelated episode, an unintegrated experience. Perspective requires that in our search for the meaning of normal the individual must be assessed in proper relation to the total population of which he or she is a member. Only thus can we render our judgments in impersonal terms, and not in the manner in which we judge, let us say, a favorite melody, that which is entwined with our memory of Clementine, or any other. And thus must one who is conceived by those who remember her as normal in nearly all, if not all respects, be viewed lest we fall into that deceptive enchantment which distance, time or personal predilection cast upon our unwary faculties. If there seems to be something heartless in my coldly analytic attitude, in my insistence that Clementine, who in our memories approaches the ideal, be judged by comparison with mortals, it is not because I lack affection or loyalty, but because these are not the instruments by which we will achieve our goal.

The all-important data, the so-called vital statistics, are things that we rarely gather about our friends. From some perversity in human nature we accumulate fewer vital statistics about our friends than about our enemies. I am in the unfortunate position of not having at my command all the vital statistics that I would we possessed about Clementine. For such facts as I do possess I am indebted to Mr. Percy Montrose, who reduced them to immortal verse in 1884. Mr. Montrose, I regret to say, has apparently been lost to history, a diligent search having failed to disclose his origin or his end. But the charm of his meter and rhyme, if I may venture so bold a metaphor, are of a parity with the charm of the young lady whose sorry fate has stirred thousands of men to throaty and tearful song.

Although Mr. Montrose has given us many facts, I have been forced to supplement his all too brief biography by careful inquiry among mutual friends. Allowing for reasonable error, but error which I am sure will not vitiate our final judgment, I think that most of the lacunae in the documented history can be filled.

Light she was, and like a fairy,

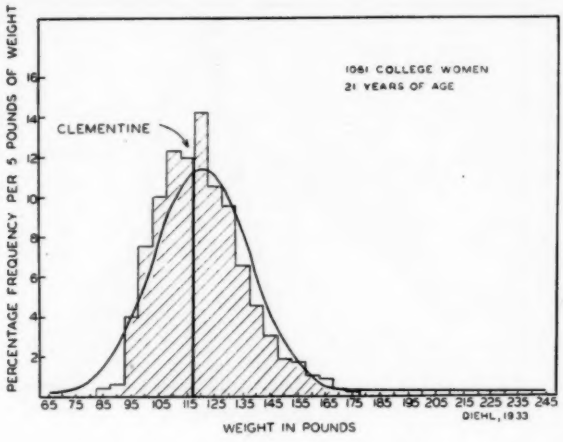


Fig. 4

is of course in part poetic extravagance on the part of her inspired biographer, and yet, perhaps not so much of an extravagance at that. I do not know Clementine's age at the time of reference, and like so many statistical data the time of reference to one detail may not be simultaneous with purportedly related data on other matters, but assuming that the impression here epitomized in a classic simile is to be taken seriously, and weighing carefully the considered assessments of my advisors, I deduce with some confidence that her weight was approximately 117 pounds at or about the age of twenty-one.

Is this—or was this—weight normal? As I have said, we must not allow either the enchantment of time or the persuasion of predilection to sway our opinion in such a matter. The only unprejudiced approach is to refer this datum on weight to the normal frequency distribution curve (Figure 4) showing the weights of females in her age group.

One thousand and sixty-one American college women twenty-one years of age[1] should certainly afford us a sound sample of the normal. The range in weight is from 65 to 245 pounds, a broad spread for the normal however you look at it. The mean is 120.7 pounds, but not a single woman weighed exactly that amount. The high frequency of women weighing between 95 and 122 pounds is balanced by a relatively few women of large weight, or moment, as the mathematician says, where the curve is skewed to the right. Nevertheless, the frequency distribution corresponds closely to the normal curve, indicating

As Aristotelians we have agreed that it is the substantive things of life, the specific objects with mass and size and color, particular persons whom we know, that supply the proper data for scientific inquiry and philosophy. But biological variation is also a fact, and without the background of a frequency distribution curve a single individual is only an unrelated episode, an unintegrated experience. Perspective requires that in our search for the meaning of normal the individual must be assessed in proper relation to the total population of which he or she is a member. Only thus can we render our judgments in impersonal terms, and not in the manner in which we judge, let us say, a favorite melody, that which is entwined with our memory of Clementine, or any other. And thus must one who is conceived by those who remember her as normal in nearly all, if not all respects, be viewed lest we fall into that deceptive enchantment which distance, time or personal predilection cast upon our unwary faculties. If there seems to be something heartless in my coldly analytic attitude, in my insistence that Clementine, who in our memories approaches the ideal, be judged by comparison with mortals, it is not because I lack affection or loyalty, but because these are not the instruments by which we will achieve our goal.

The all-important data, the so-called vital statistics, are things that we rarely gather about our friends. From some perversity in human nature we accumulate fewer vital statistics about our friends than about our enemies. I am in the unfortunate position of not having at my command all the vital statistics that I would we possessed about Clementine. For such facts as I do possess I am indebted to Mr. Percy Montrose, who reduced them to immortal verse in 1884. Mr. Montrose, I regret to say, has apparently been lost to history, a diligent search having failed to disclose his origin or his end. But the charm of his meter and rhyme, if I may venture so bold a metaphor, are of a parity with the charm of the young lady whose sorry fate has stirred thousands of men to throaty and tearful song.

Although Mr. Montrose has given us many facts, I have been forced to supplement his all too brief biography by careful inquiry among mutual friends. Allowing for reasonable error, but error which I am sure will not vitiate our final judgment, I think that most of the lacunae in the documented history can be filled.

Light she was, and like a fairy,

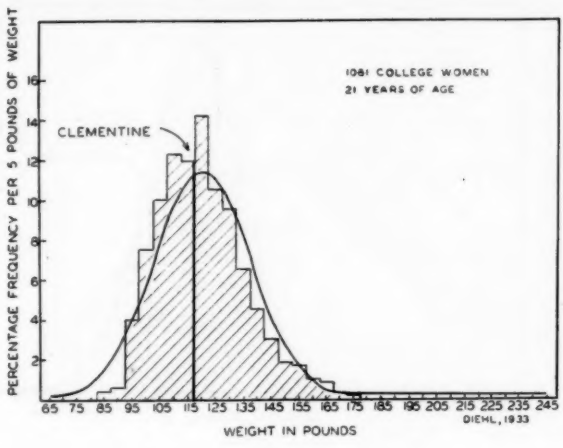

PERCENTAGE FREQUENCY PER 5 POUNDS OF WEIGHT
1081 COLLEGE WOMEN
21 YEARS OF AGE
CLEMENTINE
65 75 85 95 105 115 125 135 145 155 165 175 185 195 205 215 225 235 245
DIEHL, 1933
WEIGHT IN POUNDS

Fig. 4

is of course in part poetic extravagance on the part of her inspired biographer, and yet, perhaps not so much of an extravagance at that. I do not know Clementine's age at the time of reference, and like so many statistical data the time of reference to one detail may not be simultaneous with purportedly related data on other matters, but assuming that the impression here epitomized in a classic simile is to be taken seriously, and weighing carefully the considered assessments of my advisors, I deduce with some confidence that her weight was approximately 117 pounds at or about the age of twenty-one.

Is this—or was this—weight normal? As I have said, we must not allow either the enchantment of time or the persuasion of predilection to sway our opinion in such a matter. The only unprejudiced approach is to refer this datum on weight to the normal frequency distribution curve (Figure 4) showing the weights of females in her age group.

One thousand and sixty-one American college women twenty-one years of age[1] should certainly afford us a sound sample of the normal. The range in weight is from 65 to 245 pounds, a broad spread for the normal however you look at it. The mean is 120.7 pounds, but not a single woman weighed exactly that amount. The high frequency of women weighing between 95 and 122 pounds is balanced by a relatively few women of large weight, or moment, as the mathematician says, where the curve is skewed to the right. Nevertheless, the frequency distribution corresponds closely to the normal curve, indicating

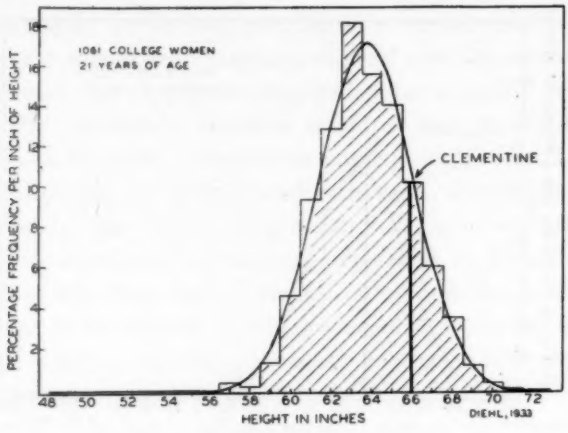

1081 COLLEGE WOMEN
21 YEARS OF AGE
CLEMENTINE
PERCENTAGE FREQUENCY PER INCH OF HEIGHT
HEIGHT IN INCHES
48 50 52 54 56 58 60 62 64 66 68 70 72
DIEHL, 1933

Fig. 5

that the factor or factors which make for inequality of weight in American college women largely enjoy independent variation and operate by pure chance, at lease in this sense: the circumstance that one college woman is light or heavy does not influence the probable weights of her classmates. Or does it? May not the circumstance that one college woman is slender lead other college women to dietary restrictions, and thus squeeze the histogram to the left and out of the boundaries of the normal curve? We will later raise the question whether the circumstances of going to college may influence the weights of all the women who do so.

Clementine, who, in a manner of speaking, has been in every college in the United States, weighed less than the mean by 0.216σ, indicating that 83 per cent of the population can be expected to deviate from the mean more than she does. We have taken as our normal standard of reference "the weights of college women 21 years of age"—by that definition Clementine is normal in that she is consistent with the standard in respect to weight.

The question arises, however, whether it is adequate in judging normality to use only one criterion of reference, weight, so let us try a second criterion, namely, height (Figure 5). Secondary sources which I need not detail. but which when collated with other data are wholly convincing, lead me to set Clementine's height as close to 66 inches. Thus she was above the average height by 1σ, indicating that 32 per

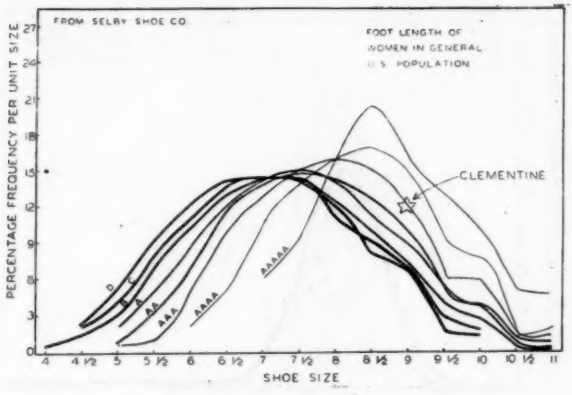


Fig. 6

cent of the population can be expected to deviate from the average height more than she does. She is still consistent with our standard of normality. The only other thing we can deduce from this additional datum is that she was slender—tall for her weight—statistics have a teasing way of proving what one already knows!

But is it abnormal to be slender? This is a matter of the correlation of two variables. Do they simultaneously enjoy independent variation, or do they tend to wax and wane together? The answer in this case is that they do not enjoy simultaneous independent variation but tend to vary in proportion to each other. Thus we are spared extreme skinniness in very tall women, and complete rotundity in very short women. The statistician, that ingenious person who is guiding us through this labyrinth, has integrated the two variables by calculating what he calls "body-build" which is equal to weight x 1000/h.^{2} According to this quotient, which affords quantitative, mathematical precision to what the eye can only qualitatively assess, Clementine is still consistent with being a college woman. In this case, multiplying one reasonable certainty by another has only led to a third reasonable certainty, but this manipulation of figures calls to mind that it is yet to be proved, certain physicists to the contrary, that a mathematician can get anything out of an equation that he has not put into it, other than the leisure which is made available by any labor-saving device.

Let us try a third criterion—foot size (Figure 6). It is something of a surprise that though most women's colleges measure something, and

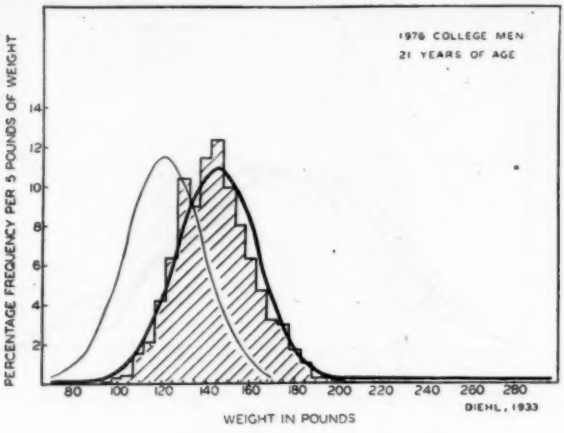


Fig. 7

many of them measure almost everything that is measurable about women, no one of them seems to have taken any interest in the size of feet. So for our standard of reference we must turn to the sizes of shoes bought by women in the general population.[2] The modern prejudice in favor of wearing shoes may be discounted as merely modish or even abnormal, and it is probable that shoes were rarely worn in the remote gold mining camps of California in the early '50's. However, Clementine was adverse to walking barefooted over the rocky trails, for, as Mr. Montrose tells us,

Herring boxes, without topses
Sandals were for Clementine.

An authority on herring boxes of that period advises me that they were of course variable in size, but generally of a width of some three inches. This datum, combined with her body build, leads me to infer that she wore a AAA shoe. The matter is, however, of slight importance—if the width of Clementine's foot did not exceed a AAAAA, she is still consistent with what we have chosen as our normal standard of reference.

It will be recalled that we initially took as our standard of normality 1061 American college women. But can we be confident of the reliability of any population of women as a normal standard of reference? Should we not examine them against a broader background before accepting them as a sample of the absolute? One method that suggests itself is to compare them with college men of the same age (Figure 7).[3]

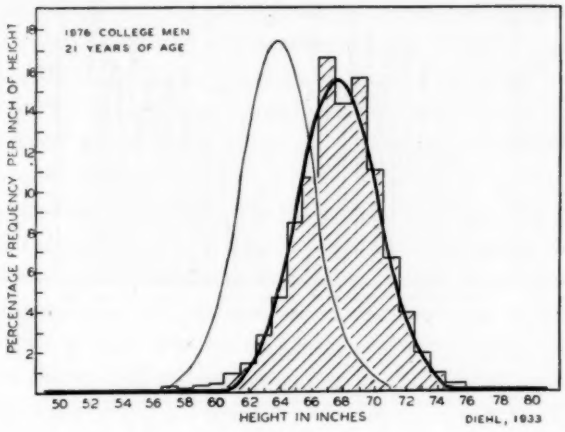


Fig. 8

The range in weight is substantially greater than among women, 68 to 298 pounds as compared with 65 to 245 pounds.[3] Moreover, the frequency distribution curve for weight is somewhat wider for men than for women, i.e., the variation or dispersion about the mean is greater. The degree of variation about the mean is expressed by σ and appropriate comparison of σ for the two curves shows that women are significantly more uniform than men, they conform more closely to a hypothetical stable ideal, as they doubtless have always thought. But does this greater stability in weight make them more normal than males? There is an aphorism from the library of Assurbanipal, king of ancient Nineveh, which asks, "Does a woman conceive when a virgin, or grow fat without eating?" Voluntary dietary restriction is certainly not normal.

There are, of course, other factors operating, as is evident when we compare men with women with respect to height (Figure 8). Here again women are more stable, they show less variation about the mean. To understand how this may come about let us compare height and weight. In both sexes the variation in height is much less than in weight; height is a much more stable feature of the human body than weight. If the body were shaped roughly like an egg, its mass would increase as the cube of its height, a circumstance which itself would make for greater variation in weight than in height, even among ideal eggs. But neither men nor women can be conceived as behaving like ideal eggs; in both sexes weight is largely a matter of soft tissue which can be

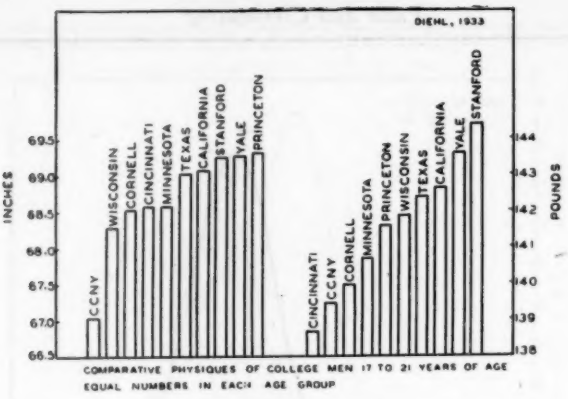


Fig. 9

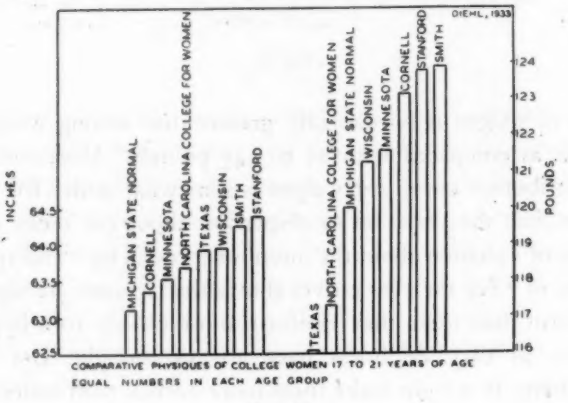


Fig. 10

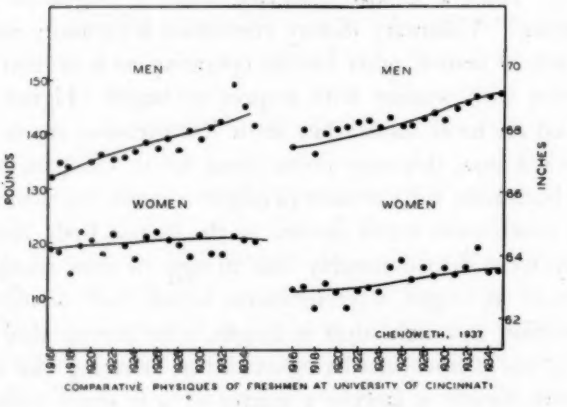


Fig. 11

added or subtracted by indulgence or hardship. Height is largely a matter of long bones and the vertebral column, the dimensions of which are less susceptible to fluctuations in dietary fortune, but are largely determined by the pituitary gland. Are women more normal than men because they agree about pituitary secretion among themselves, but disagree with men?

The question has arisen whether going to college affects the parameters under consideration. It does, in advance of matriculation. Both men and women who enter most colleges are taller and heavier than the non-collegiate general population. College women at the age of 16 average five pounds heavier and over one inch taller than their non-collegiate sisters, though they are four pounds lighter at the age of 21. Unlike women at large, college women do not increase in either weight or height between 16 and 21. It has been suggested that restriction of diet may retard their growth, but I have not observed that anyone has considered a possible causal relationship to the college diet itself, or, even less remotely, the effects of the intellectual life. Similarly college men are taller and heavier than the general population. Apparently, in the college group acceleration in development has occurred prior to the age of 16, so that by the age of 17 or 18 years they have more nearly approached full growth (Figure 9). But the situation is not the same in all colleges; among men, Stanford and California are exceeded in height only by Yale and Princeton, while in respect to weight only Yale is a competitor. C.C.N.Y. comes out short in both respects (Figure 10). Among women, Stanford comes out first in height, but in weight is beaten by an ounce by Smith. Our criterion of normal depends on what college we pick. Is this predilection of a few colleges for mass and length normal?

These general differences awaken us to a new difficulty, that of finding a stable population. Numerous studies have revealed that over the past century and even in the past few decades, there has been an acceleration of growth and maturation. In several regions menarche occurs two years earlier than a century ago, and in three American Universities the mean menarchial age has dropped by one year in the last thirty years. The average height of English soldiers has increased about 4 inches in the past century, that of American college men by about 2 inches in the last half century, that of women only slightly less (Figure 11). For several generations children have quite generally ex-

ceeded their parents (of like sex) in stature and earliness of puberty, and berth length in American sleeping cars has been repeatedly lengthened, from 71 inches (center to center of partitions) in 1859 to 77.5 inches actual mattress length in many cars now being made.[4, 5, 6] Diet may have played a part in this acceleration and extension of growth, but some investigators[7] relate the phenomenon to a period of reduction in the temperature of the earth, and believe that the growth and development peak may have been reached with rising temperature in the 1930's, and that future decades are likely to witness a racial recession.

We have asked ourselves to what extent can our ideal, Clementine, be conceived as approaching the normal in respect to any one of several mensurable variables. We may measure Clementine, but we have not as yet measured the normal. If the word is to have any meaning it will have to be in reference to some selected population. In the face of the above facts it is clear that we cannot lay our hands upon a stable population. On what grounds, then, can we call anything human, normal, unless we call everything human, normal?

Perhaps we have been searching in the wrong direction—perhaps the mere dimensions of things do not comprise their essence. Perhaps we should seek in the dynamic, rather than the static, to discover the normal. One of our colleagues, a neuroanatomist, has in effect already made this search in terms of function. Rejecting, as we have, the average as a mere mathematical computation, a specious name representing no reality, he defines the normal as "that which functions in accordance with its inherent design."[8] He suggests a simple example: if we come across a broken wheel it is possible to infer not only the inherent function of the object, but in addition what is wrong or abnormal about it. ". . . the essence of (its) design is circularity including the presence of an axis . . . (and) the function potentially associated with circularity in a discrete object is revolution." To those who may object that we learn the relationship between circularity and rolling by experience, and therefore by the law of averages, he replies with assurance that we can recognize a broken wheel the first time we ever see one, even though we have had no other experience with wheels. In this nascent experience the thing is not a novel object of unaccustomed design and function; we are under no disability whatever to recognize either that it is a wheel, with at least the implication of rotation, or that the local departure from design which prevents the fulfillment of the wheel's natu-

ral function is to be accounted for by the circumstance that the wheel has been broken. Were this not true, he says, the first wheel could never have been either recognized or invented.

The origin of the wheel is wholly obscure. Whether it was discovered by a perspicacious inventor who shared with our author this inborn insight into the relationship between design and function, or by a comparatively stupid dunce who fell upon the idea by slipping on a round stone or log, i.e., by the law of averages, I do not know. But I do not think that to define normal as the relationship between design and function is going to get us far. If it is normal for an intact wheel to roll in accordance with its design, is it not equally normal for a broken one to fail to roll, in accordance with *its* design? And why must a wheel roll, anyway? Only a boy who wants to play hoops, only a lazy man unwilling to carry his load or to walk from place to place, or who finds in the relationship of circularity to rotation a tempting teleological argument, would insist that a wheel must roll. Is a wheel not functioning properly when it lies flat upon the ground? Many a chipmunk thinks so. Our definition of normal must not be subordinate to human usefulness. We appear to be in a blind alley—indeed I think we are slipping backwards, for let us see to what conclusions the author's own anthropocentric argument leads him:

"We intend to assert," he says, "a direct, but largely unremarked, implication of biology, viz., that the organic design of the human being is the one complete design of its kind existing within the entire organic kingdom on this planet. The detailed arguments on which the statement rests are too lengthy for present reproduction but it is felt that almost any biologist must agree with them upon reflection. They are based not only upon structural considerations but on functional ones as well; no other creature behaviorally manifests the integration of intellectual, affective, and sensori-motor response which characterizes man or, if any do, then by no means to the same degree of completeness. Thus the human design represents the complete and fundamental organic pattern from which other species depart by one degree or another, and the basic paradic [= normal] of the organic kingdom is the anthroparadic. . . . On the basis of completeness the anthroparadic must be the ultimate paradic of the whole organic kingdom."

Oh, Clementine. . . ! How little did you suspect that you were the "paradic," the paradigm, the norm for the whole organic kingdom! But

do not be alarmed by this apotheosis. . . . These arguments are not new despite the fact that the paper I have quoted was published in the year of diminishing grace, 1945. Christian theologists, beginning with Augustine, elevated man to the cosmic dais, only they modestly recognized that man had a few minor imperfections and so under the doctrine of the Perfection of the Original Creation (prior to Adam's sin, of course, on which occasion the serpent tempted Eve (who was weak and perhaps abnormal) and Eve tempted her husband and because of Adam's sin Creation was despoiled)—because of these things they pointed to Adam, and not to Adam's progeny, as the paradigm of Creation. I would that we could do the same. I would that we had Adam—I mean no disrespect—on our dissecting table so that we could study all his "paradic" parameters, the width and breadth of his skull, the weight of his brain and heart and kidneys; I would that we could even make a few observations on his cardiac output and his renal clearances. . . . No Clementine, you are, if I may say it without being misunderstood, no Eve, and in any case, even the doctrine of Original Perfection and the legend of Adam and Eve have been heavily discounted since the publication of Mr. Darwin's book, since man has been revealed to be something less than a fallen angel and inadequate to serve as a model for the whole of organic creation. . . .

Have we, then, in our search for the normal, become lost in an endless labyrinth? Let us return to where we started, the stability of type that pervades the entire organic kingdom—it has been this stability of type coupled with the Platonic Ideal that has fostered our illusion that the word normal has any meaning. But is this stability of type real, does nature have a common template upon which she patterns every starfish, every oyster, every man? We know more about men than about oysters and starfish, and for man we can confidently answer, No. The high improbability that no two men are exactly alike in their chromosomal pattern (except in the case of identical twins) is one of the best established facts of biology. Every man differs from every other man not only in his finger prints but doubtless in a thousand other characters. If no two men are exactly alike in their chromosomal pattern, then where is the normal man?

The concept of the normal is an illusion of our own making. It is not easy to abandon one's illusions. Those who walk among the sick ask themselves many times a day, is this normal or abnormal? The

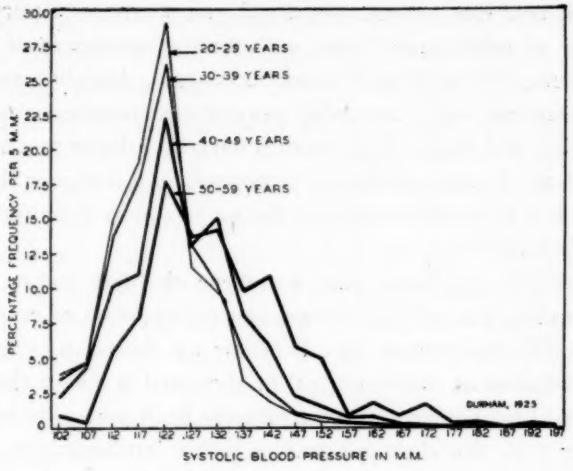


Fig. 12

physician who would prevent rather than cure disease must carry the question into that no-man's land where disease is nascent and difficultly discoverable—where does the normal end and disease begin? We are engaged in an issue between life and death and we cannot define our enemy—what is disease? Is it not normal for some men to be infected with non-pathogenic pneumococci—do we not call it abnormal only when the organism is pathogenic? But do we gain anything thereby? What does it aid us to define pneumonia as abnormal when we are not embarrassed to think that a pneumococcus is a normal organism that follows a normal course of reproduction in somebody's alveoli, to the victim's inevitable distress and until he dies—unless treated with sulfanilamide—all in accordance with laws apparently as unchanging as Plato's universals, the whole a normal sequence of events? Is it not normal for a diabetic to exhibit glycosuria, for a cretin to be just what a cretin is, for a malignant tumor to do just what a malignant tumor does, for a schizophrenic to behave like a schizophrenic?

Let us take one more example of a physiological variable where we habitually use the word normal—blood pressure (Figure 12). Where will we set the limits of normal blood pressure? To do this in an arbitrary manner would be not only intellectually dishonest, but dangerous, for these blood pressure frequency distribution curves are charged with pain, disability and death. . . .

Is it not true that we are concerned, not with the preservation of tradition or of aristocratic rights, or with the apotheosis of an Ideal blood pressure, but with these brute facts—pain, disability and death? And are there not many ostensibly normal circumstances that lead to pain, disability and death? Senescence is normal—I doubt that any biologist will deny it—and childbirth is normal, and between birth and senescence it is impossible to define disease except in these terms—pain, disability and death.

Our task is to ameliorate pain, to relieve disability and to postpone death by reading the future in the present constellation of events, in the hope of modifying future events. Whether we choose to call any particular constellation of events normal or abnormal is not of the slightest significance. That we have in some measure been successful in our task of avoiding pain, disability and death is partly attributable to our great theory of disease, stemming largely from Cohnheim, namely, that singular, particular causes underlie disease. If a man has pneumonia, it is because a specific noxious agent has invaded his body and made headway against his defenses. And so with the deficiency diseases, resulting from the absence from the body of particular, necessary elements; and here, too, one can place malignancy: particular body cells, perhaps only one cell in the beginning, have escaped from control and by accelerated development and overgrowth destroyed vital organs. And here, too, one can place many inheritable diseases, as well as established predispositions to disease—modern genetics permits us to believe that in all such cases a particular fault in one or a few chromosomes may be responsible.

Under this particularistic theory it has been relatively easy to slip into the error of dividing everything into the normal and abnormal. But as opposed to or supplementing a theory which looks always for simple causes (and is confirmed by always finding them complex), is it not conceivable that there can be disease which may not be so particularistic in its genesis, nor yet necessarily inheritable? Suppose we examine the frequency distribution of a well shuffled pack of fifty-two cards. There are only four chances in 635,013,559,600 of being dealt thirteen cards of one suit, and yet when such a hand falls upon the table it falls in accordance with all the rules that govern the fall of cards in a common or modal hand. It is as much a normal hand as any other, even though a very rare one. Suppose those fifty-two cards were so many chromosomal determinants of somatic characters, none of which is very

obvious or very important in itself; but suppose that in rare combinations, like thirteen cards of one suit, they spelled jeopardy for the unlucky player. The organism is in perpetual conflict with its environment and what is one man's environment may be another man's ruin. It has been said that God must love the common man because he made so many of them: may we not suspect that there are more individuals at the mode of biological variation because they are better fitted to survive in a specified environment than are the individuals at either extreme of the range? The man who gets too many of any one suit in his genetic makeup may be destined for an early death because of his inability to cope with the stress of his environment. Yet by all the rules he has had a normal deal. His obscure pathology need not be inherited by his children, nor could one point to a particularistic cause such as pneumococcus, a vitamin deficiency, a tumor or a default in some particular gland, as the primary cause of his distress. Can we name such diseases? Perhaps senescence is such a disease, a matter of too many, or too few clubs. Perhaps there are others. Perhaps we have looked so hard for particularistic causes under Cohnheim's influence that we have found them where they do not exist. Perhaps by classifying everything as normal or abnormal under Plato's influence we have overlooked other possibilities. Perhaps the pathologist should take the theory of probability into his laboratory and apply it to ostensibly normal people.

And there's that word again. The Platonic Ideal lives on in our Aristotelian house of science, a formless, limitless, insubstantial ghost not to be captured by any trap set by the anatomist, physiologist or statistician. There is no such thing as an ostensibly normal person—only persons who show no ostensible danger of pain, disability or death. The word normal is useful in only one sense—as a tag for ignorance.

It is a paradox that when probability came into philosophy, freedom was expelled. Causality took over our lives and our innermost thinking, and even disease and death were constrained to run a deterministic course. Environment, circumstance, and constitution—these three became our Fates. No better example could be adduced than that tragedy, reference to which I have hitherto avoided: as Mr. Montrose recounts of Clementine, in the manner of those who depend for their sustenance on Nature's bounty and who must discharge their obligations to Nature's creatures promptly and regularly,

Drove she ducklings to the water
Every morning just at nine,

Here, in environment and circumstance, was the fatal pattern laid. Though I cannot exclude the possibility that constitution in the matter of her foot and footwear accelerated the mischance, when there is sufficient repetition probability becomes dead certainty, and one morning Clementine

Hit her foot against a splinter
Fell into the foaming brine.

I am sorry that at this point I must interject a harsh note of criticism of our biographer for committing a solecism as indefensible as it is shocking. Fully recognizing the demands of meter and rhyme in poetic form, and appreciating the difficulties of discovering another noun adequate in respect to both context and euphony, I fail to understand how Mr. Percy Montrose could have penned that iambic tetrameter. That the cool rivulets of condensed cloud trickling between mossy stones high on the Sierra Nevada, the springs which well forth from canyon wall or glacier's lip, the gurgling brooks and torrential streams which tumble and toss down the deep-hewn, shadowed gorges of California's mountains—that any one of these crystal cataracts should be called *brine* is an offence against truth and taste from which I cannot recover.

I would also like to place on record my conviction that the sixth and seventh verses, commonly appended at the end of the musical score as though to conserve space, are in truth removed from the manuscript proper for a better reason. Careful study of internal evidences convinces me that these verses, the one relating the death from sorrow of Clementine's father, and the one starting "In my dreams she still doth haunt me," are in fact spurious, apocryphal forgeries by a later hand attempting to imitate Mr. Montrose's rhythm, but rendered indelicate by too many trips to the beer keg. And I may also note that, according to my authentic record, in the last bar of the chorus the melody does not follow the frequently heard (if not inharmonious) course from C to E, but repeats high C precisely in the manner of the verse.

But these details are unimportant. My point is that, not in the normal or abnormal, but in environment, circumstance and constitution is concealed our potential fate, and each of these is complexly manifold and complexly changing. Against the potentialities of fate we must oppose calculated action.

Ruby lips above the water,
Blowing bubbles soft and fine,
A-las, for me! I was no swimmer,
So I lost my Clementine.

We must abandon our dream of the absolute, the unchanging, which began when Pythagoras created the world of eternal numbers, and Plato of eternal Ideas. It is required of us that we find that which is significant not in the absolute, but in diversity and change and transmutation.

In a churchyard, near the canyon,
Where the myrtle doth entwine,
There grows roses and other posies,
Fertilized by Clementine.

Our search for the Ideal, the paradigm, the pattern of perfection, has led us only to disillusionment because that which we seek is but the product of our dreams. The normal is gone because it never was.

O my darling, O my darling,
O my darling Clementine,
You are lost and gone forever,
Dreffful sorry, Clementine.

If we cherish the Ideal the more because we cannot attain it, because nature has never attained it, this poignant craving but serves to fire us with greater zeal and fidelity to our proper companion in philosophy, the substantive, despite her imperfections and the fact that she is never twice the same. Some modern but unknown philosopher has captured the nuance of the Platonic-Aristotelian transformation in a quatrain which perhaps you know, and love:

How I missed her, how I missed her,
How I missed my Clementine,
'Til I kissed her little sister,
And forgot dear Clementine.

REFERENCES

1. Diehl, H. S. Heights and weights of American college women, *Human Biol.*, 1933, *5*:600.
2. From data issued by Selby Shoe Company, Portsmouth, Ohio.
3. Diehl, H. S. Height and weights of American college men, *Human Biol.*, 1933, *5*:445.
4. Mills, C. A. Geographic and time variations in body growth and age at menarche, *Human Biol.*, 1937, *9*:43.
5. Chenoweth, L. B. Increase in height and weight and decrease in age of college freshmen over a period of 20 years, *J.A.M.A.*, 1937, *108*:354.
6. MacKinnon, D. C. and Jackson, C. M. Changes in physical measurements of male students at the University of Minnesota during the last 30 years, *Am. J. Anat.*, 1931, *47*:405.
7. Mills, C. A. and Chenoweth, L. B. Is human growth tide turning? *Human Biol.*, 1938, *10*:547.
8. King, C. D. The meaning of normal, *Yale J. Biol. & Med.*, 1945, *17*:493.

RECENT ACCESSIONS TO THE LIBRARY

("Possession does not imply approval.")

Books

Asratyan, E. A. Ocherki po etiologii, patologii i terapii travmaticheskogo shoka. [Outlines on the etiology, pathology and therapy of traumatic shock.] Moskva, MEDGIZ, 1945, 173 p.

Brown, (Sir) W. L. Some chapters in Cambridge medical history. Cambridge [Eng.], University Press, 1946, 119 p.

Brusselmans, P. De ziekten van de lever en van de galwegen. Antwerpen, Standaard-Boekhandel, 1944, 98 p.

Cantor, A. J. Ambulatory proctology. N. Y., Hoeber, [1946], 524 p.

Conference on Fertility. New York, 1946. The problem of fertility; proceedings of the Conference . . . held under the auspices of the National Committee on Maternal Health. Princeton, N. J., Princeton Univ. Press, 1946, 254 p.

Conference on Preventive Medicine and Health Economics, Ann Arbor, 1946. Proceedings. [Ypsilanti, University Lithoprinters, 1947], 245 p.

Cooke, R. A. Allergy in theory and practice. Phil., Saunders, 1947. 572 p.

Delay, J. P. L. Les dérèglements de l'humeur. Paris, Presses Universitaires de France, 1946, 179 p.

Delay, J. P. L. L'électro-choc et la psychophysiologie. Paris, Masson, 1946, 169 p.

Dewar, T. A textbook of forensic pharmacy. London, Arnold, [1946], 253 p.

Dingman, H. W. Risk appraisal. Cincinnati, National Underwriter Co., 1946, 824 p.

Eckman, J. Jerome Cardan. Balt., Johns Hopkins Press, 1946, 120 p.

Entin, D. A. Voennaya chelyustno-litsevaya khirurgiya. [Military surgery of the jaws and face.] 3. izd. Moskva. MEDGIZ, 230 p.

Fanconi, G.; Zellweger, H. & Botsztejn, A. Die Poliomyelitis und ihre Grenzgebiete. Basel, Schwabe, [1945], 694 p.

Filatov, V. P. Opticheskaya peresadka rogovitsy i tkanevaya terapiya. [Optic transplantation of the cornea and therapeutic tissue transplantation.] Moskva, MEDGIZ, 1945, 231 p.

Fischer, A. Biology of tissue cells. Cambridge [Eng.], University Press, 1946, 348 p.

Fitzsimmons, L. W. Textbook for psychiatric attendants. N. Y., Macmillan. 1947, 332 p.

Florkin, M. & Massart, L. Medische biochemie. Antwerpen, Standaard-Boekhandel, 1944, 641 p.

Franz, C. A. O. Lehrbuch der Kriegschirurgie.. 4. Aufl. Berlin, Springer, 1944, 475 p.

Gross, M. Acetanilid; a critical bibliographic review. New Haven, Hillhouse, 1946, 155 p.

Gulevich, V. S. Analiz mochi. [Analysis of urine.] 3. izd. [Leningrad, MEDGIZ], 1945, 239 p.

Hamilton-Paterson, J. L. Penicillin in general practice. London, Staples, [1946], 95 p.

Harris, S. Banting's miracle; the story of the discoverer of insulin. Toronto, Dent, [1946], 245 p.

Hartford Medical Society. The first century of the Hartford Medical Society, 1846-1946. Hartford, Conn., Hartford Med. Soc., 1946, 94 p.

Holmes, G. M. Introduction to clinical neurology. Edinburgh, Livingstone, 1946, 183 p.

Hyman, H. T. An integrated practice of medicine. Phil., Saunders, [1946-1947], 4 v. & index.

Iason, A. H. The thyroid gland in medical history. N. Y., Froben, 1946, 130 p.

Kardiner, A. & Spiegel, H. X. War stress and neurotic illness. [2. ed.] N. Y., Hoeber, [1947], 428 p.

Kerr, D. J. A. Forensic medicine. 4. ed. London, Black, 1946, 359 p.

Lederer, F. L. Diseases of the ear, nose and throat. 5. ed. Phil., Davis, 1946, 934 p.

Lipovetz, F. J. Medical physical education. Rev. ed. Minneapolis, Burgess, 1946, 417 p.

Lips, A. C. M. & Verschure, J. C. M. Vorderingen der geneeskunde. Amsterdam, Scheltema, 1946, 680 p.

Marmelszadt, W. Musical sons of Aesculapius. N. Y., Froben, 1946, 112 p.

Mengert, W. F. Postgraduate obstetrics. N. Y., Hoeber, [1947], 392 p.

Morhardt, R. P. Handbook of death; mechanisms, causes and certification. South Pasadena, Morhardt, [1946], 376 p.

Murphy, D. P. Uterine contractility in pregnancy. Phil., Lippincott, [1947], 134 p.

Nasio, J. Tratamiento médico de la úlcera gastroduodenal experimental. Buenos Aires, [La Prensa Médica Argentina], 1946, 151 p.

Novak, E. Gynecological and obstetrical pathology. 2. ed. Phil, Saunders, 1947, 570 p.

Pelner, L. Gastroenterology in general practice. Springfield, Ill., Thomas. [1946], 285 p.

Penicillin; its practical application, under the general editorship of Sir A. Fleming. London, Butterworth, 1946, 380 p.

Petit-Dutaillis, D. & de Sèze, S. Sciatiques & lombalgies par hernie postérieure des disques intervertébraux. Paris, Masson, 1945, 178 p.

Progress in gynecology, edited by J. V. Meigs and S. H. Sturgis. N. Y., Grune, 1946, 552 p.

Ranson, S. W. The anatomy of the nervous system. 8. ed. Phil., Saunders, 1947, 532 p.

Robinson, V. Victory over pain; a history of anesthesia. N. Y., Schuman, [1946], 338 p.

Rosenfield, I. Hospitals; integrated design. N. Y., Reinhold, [1947], 308 p.

Short. A. R. The causation of appendicitis. Bristol, Wright, 1946, 79 p.

Sinai, N.; Anderson, O. W. & Dollar, M. L. Health insurance in the United States. N. Y., Commonwealth Fund, 1946, 115 p.

Stedman, T. L. Practical medical dictionary. 16. ed. Balt., Williams, 1946, 1291 p.

Steinhaus, E. A. Insect microbiology. Ithaca, Comstock, 1946, 763 p.

Vásquez, J. R. Nuevas orientaciones en la dietética del lactante (alimento de Bessau). Buenos Aires, El Ateneo, 1946, 125 p.

Vonderlehr, R. A. & Heller, J. F. The control of venereal disease. N. Y., Reynal, [1946], 246 p.

Wesson, M. B. Urologic roentgenology. 2. ed. Phil., Lea, [1946], 259 p.

Monographs in Series, etc.

Acklin, O.; Rossi, E. & Schmid, M. Zur Chemotherapie der Sulfone gegen Tuberkulose. 25 p. In: Helvetica Paediatrica Acta, Basel, 1946, suppl. 3.

Alha, A. R. Studies on the relative resistance of germs to the salts of metals of group I of the periodic table. 74 p. In: Acta Pathologica et Microbiologica Scandinavica, København, 1946, suppl. 65.

Carlgren, L. E. Gallop rhythm in children studied by means of calibrated phonocardiography. 141 p. In: Acta Paediatrica. Lund, 1946, v. 33, suppl. 6.

Engström, A. Quantitative micro- and histochemical elementary analysis by roentgen absorption spectography. 106 p. In: Acta Radiologica, Stockholm, 1946, suppl. 63.

Frey, T. S. Electroencephalographic study of neuropsychiatric disorders. 171 p. In: Acta Psychiatrica et Neurologica, Stockholm, 1946, suppl. 42.

Gard, S. Purification of poliomyelitis viruses. 173 p. In: Acta Medica Scandinavica, Uppsala, 1943, suppl. 143.

Gil Espinosa, M. Ueber nicht hypophysäre Chiasmasyndrome. 60 p. In: Bibliotheca Ophthalmologica, Basel, 1946. fasc. 32.

Lundgren, N. P. V. The physiological effects of time schedule work on lumberworkers. 137 p. In: Acta Physiologica Scandinavica, Stockholm, 1946, suppl. 41.

Porjé, I. G. Studies of the arterial pulse wave, particularly in the aorta. 68 p. In: Acta Physiologica Scandinavica, Stockholm, 1946, suppl. 42.

Ramer, K. T. The prognosis of mentally retarded children. 142 p. In: Acta Psychiatrica et Neurologica, Stockholm, 1946, suppl. 41.

Reubi, F. Le traitement de la néphrite aiguë par les antihistaminiques de synthèse. 50 p. In: Helvetica Medica Acta, Basel, 1946, suppl. 18.

Ritter, A. Katgut. 68 p. In: Helvetica Chirurgica Acta, Basel, 1945, suppl. [1].

Saarenmaa, E. J. Investigations of the bacterial flora, disinfection. and disinfectants of the skin. 150 p. In: Annales Chirurgiae at Gynaecologiae Fenniae, Helsinki, 1946, v. 35, suppl. 4.

Seipel, C. G. M. Variation of tooth position. 176 p. In: Svensk Tandläkare-Tidskrift, Lund, 1946, v. 39, suppl.

Sorba, M. Gonadotrophines et tumeurs testiculaires. 80 p. In: Helvetica Chirurgica Acta, Basel, 1946, suppl. 2.

Thomassen, T. L. Experimental investigations into the conditions of tension in normal eyes and in simple glaucoma. 196 p. In: Acta Ophthalmologica, Copenhagen, 1946, suppl. 27.

Wissler, H. & Zollinger, H. U. Die familiäre kongenitale zystische Pankreasfibrose mit Bronchiektasien. 88 p. In: Helvetica Paediatrica Acta, Basel, 1945, suppl. 1.

Continuations

Directory of Medical Specialists, v. 3, 1946.

Directory of Social Agencies of the City of New York, 1946-1947.

Hospital Purchasing File, 24. ed., 1946-1947.

March of Medicne, 1946.

Royal College of Surgeons of England. Calendar, 1946.

Transactions of the American Surgical Association, 66th meeting, 1946.

Vitamins and Hormones, v. 4, 1946.

Year Book of the Eye, Nose and Throat, Aug., 1945-July, 1946.

Year Book of General Medicine, Aug., 1945-June, 1946.

Year Book of General Surgery, 1946.

Year Book of General Therapeutics, Nov., 1945-Aug., 1946.

Year Book of Radiology, July, 1945-June, 1946.

New Periodicals

Acção Médica; revista técnica de doutrina e critica de Associação dos Médicos Católicos Portugueses, Lisboa, ano 8, fasc. 29, 1944.

Acta Clinica Belgica, [Bruxelles], tome 1, fasc. 1, Jan./Feb., 1946.

Annales Pharmaceutiques Françaises; organe officiel des Sociétés de Pharmacie de Paris et de Province, Paris, tome 1, 1943.

Archivio "E. Maragliano" di Patologia e Clinica, Genova, vol. 1, fasc. 1/2, Jan./Apr., 1946.

Boletin de la Clinica Ginecotocológica, Cátedra del Prof. Manuel B. Rodriguez Lopez [Hospital Pereira Rossell], Montevideo, año 1, no. 1, April, 1946.

Cornell Conferences on Therapy, ed. by Harry Gold . . . [and others], New York, vol. 1, 1946.

Farmacoterapia Actual; revista científica de farmacología y técnica químicofarmacéutica, Madrid, año 1, núm. 1, July, 1944.

Ginecología y Obstetricía de México, editada por la Asociación Mexicana de Ginecología y Obstetricía, México, año 1, num. 2, Apr./June, 1946.

Health and Social Welfare, London & New York, 1944/1945.

Helvetica Chirurgica Acta, hrsg. im Auftrag der Schweizerischen Gesellschaft für Chirurgie, Basel, v. 12, fasc. 1/2, May, 1945.

Helvetica Paediatrica Acta, Basel, v. 1, fasc. 1, Aug., 1945.

Hematology Monographs, published by the Department of Hematology, Michael Reese Hospital . . . Chicago, vol. 1, no. 1, March, 1946.

Proceedings of the General Meeting of the Honorary Consultants to the Army Medical Library, [Balt.], 1, 1944.

Proceedings of the Nutrition Society [Great Britain], Cambridge, vol. 1, no. 1/2, 1944.

Professional Staff Proceedings of the Halloran General Hospital, Halloran General Hospital, Staten Island, N. Y., vol. 1, no. 1-10, 1946.

Progress in Neurology and Psychiatry; an annual review, edited by E. A. Spiegel, New York, v. [1, Dec. 1944/Nov. 1945].

Recovery Journal; a monthly magazine for the study of self-help methods in the treatment of nervous and former mental patients, Chicago, vol. 1, no. 1, June 1946.